PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCCLII.

PART I.

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MDCCCLII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

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upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

The Meteorological Journal hitherto kept by the Assistant Secretary at the Apartments of the Royal Society, by order of the President and Council, and published in the Philosophical Transactions, has been discontinued. The Government, on the recommendation of the President and Council, has established at the Royal Observatory at Greenwich, under the superintendence of the Astronomer Royal, a Magnetical and Meteorological Observatory, where observations are made on an extended scale, which are regularly published. These, which correspond with the grand scheme of observations now carrying out in different parts of the globe, supersede the necessity of a continuance of the observations made at the Apartments of the Royal Society, which could not be rendered so perfect as was desirable, on account of the imperfections of the locality and the multiplied duties of the observer.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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Dublin.
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Society of Experimental Philosophy.

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The *fifty* Foreign Members of the Royal Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Astronomical Observations (including Magnetism and Meteorology) made at the Royal Observatory at Greenwich, on making application for the same directly or through their respective agents, within two years of the date of publication.

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Wartmann, Professor Elie	Geneva.
Younghusband, Capt., R.A.	Woolwich.

ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology or Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1852 for the most important paper in Physics, communicated to the Royal Society after the

termination of the Session in June 1848, and prior to the termination of the Session in June 1851, and printed in the Philosophical Transactions.

The Council propose also to give one of the Royal Medals in the year 1852 for the most important paper in Geology or Mineralogy, communicated to the Royal Society after the termination of the Session in June 1848, and prior to the termination of the Session in June 1851, and printed in the Philosophical Transactions.

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In equations 11, 12, 13, 15, 16, 17, 18, 19, 23, 24, *for* $H_1 \mp H_2$ *read* $H_1 - H_2$.

In equation 19, *for* $\frac{1}{12}\mu$ *read* $\frac{1}{24}\mu$.

In page 620, *omit* the third, fourth and fifth lines

PHILOSOPHICAL TRANSACTIONS.

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Received and read January 15, 1852.

§ 17.

In § 3. of the first part of my "Contributions to the Physiology of Vision," published in the Philosophical Transactions for 1838, speaking of the stereoscope, I stated, "The pictures will indeed coincide when the sliding pannels are in a variety of different positions, and consequently when viewed under different inclinations of the optic axes; but there is only one position in which the binocular image will be immediately seen single, of its proper magnitude, and without fatigue to the eyes, because in this position only the ordinary relations between the magnitude of the pictures on the retina, the inclination of the optic axes, and the adaptation of the eye to distinct vision at different distances, are preserved. The alteration in the apparent magnitude of the binocular images, when these usual relations are disturbed, will be discussed in another paper of this series, with a variety of remarkable phenomena depending thereon."

In 1833, five years before the publication of the memoir just mentioned, these yet unpublished investigations were announced in the third edition of Herbert Mayo's "Outlines of Human Physiology" in the following words:—"Mr. Wheatstone has shown, in a paper he is about to publish, that if by artificial means the usual relations which subsist between the degree of inclination of the optic axes and the visual angle which the object subtends on the retina be disturbed, some extraordinary illusions may be produced. Thus, the magnitude of the image remaining constant on the

retina, its apparent size may be made to vary with every alteration of the angular inclination of the optic axes."

I shall resume the consideration of the phenomena of binocular vision with this subject, because the facts I have ascertained regarding it are necessary to be understood before entering on the new experiments relating to stereoscopic appearances which I intend to bring forward on the present occasion.

Under the ordinary conditions of vision, when an object is placed at a certain distance before the eyes, several concurring circumstances remain constant, and they always vary in the same order when the distance of the object is changed. Thus, as we approach the object, or as it is brought nearer to us, the magnitude of the picture on the retina increases; the inclination of the optic axes, required to cause the pictures to fall on corresponding places of the retinæ, becomes greater; the divergence of the rays of light proceeding from each point of the object, and which determines the adaptation of the eyes to distinct vision of that point, increases; and the dissimilarity of the two pictures projected on the retinæ also becomes greater. It is important to ascertain in what manner our perception of the magnitude and distance of objects depends on these various circumstances, and to inquire which are the most, and which the least influential in the judgements we form. To advance this inquiry beyond the point to which it has hitherto been brought, it is not sufficient to content ourselves with drawing conclusions from observations on the circumstances under which vision naturally occurs, as preceding writers on this subject mostly have done, but it is necessary to have more extended recourse to the methods so successfully employed in experimental philosophy, and to endeavour, wherever it be possible, not only to analyse the elements of vision, but also to recombine them in unusual manners, so that they may be associated under circumstances that never naturally occur.

The instrument I shall proceed to describe enables these abnormal combinations to be made in a very simple and effectual manner. Its principal object is to cause the binocular pictures to coincide, with any inclination of the optic axes, while their magnitudes on the retinæ remain the same; or inversely, while the optic axes remain at the same angle, to cause the size of the pictures on the retinæ to vary in any manner.

Two plane mirrors inclined 90° to each other are placed together and fixed vertically upon a horizontal board. Two wooden arms move round a common centre situated on this board in the vertical plane which bisects the angle of the mirrors, and about 1½ inch beyond their line of junction. Upon each of these arms is placed an upright pannel, at right angles thereto, for the purpose of receiving its appropriate picture, and each pannel is made to slide to and from the opposite mirror. The eyes being placed before the mirrors, the right eye to the right mirror and the left eye to the left mirror, and the pannels being adjusted to the same distances, however the arms be moved round their centre, the distance of the reflected image of each picture from the eye will remain exactly the same, and consequently its retinal magnitude

will be unchanged. But as the two reflected images do not occupy the same place when the pictures are in different positions, to cause the former to coincide the optic axes must converge differently. When the arms are in the same straight line, the images coincide while the optic axes are parallel; and as they form a less angle with each other, the optic axes converge more to occasion the coincidence. When the arms remain in the same positions, while the pannels slide towards or from the mirrors, the convergence of the optic axes remains the same, but the magnitude of the pictures on the retinæ increases as the distance decreases. By the arrangement described, and which is represented by figs. 1 and 2 Plate I., the reflected pictures are always perpendicular to the optic axes, and the corresponding points of the pictures, when they are exactly similar, fall upon corresponding points of the retinæ. The instrument has an adjustment for otherwise inclining them if it be required.

Let us now attend to the effects produced. The pictures being fixed at the same distance from the mirrors, there is a certain adjustment of the arms at which the binocular image will appear of its natural size, that is, the size we judge the picture itself to be when we look at it directly; in this case the magnitude of the pictures on the retinæ and the inclination of the optic axes preserve their usual relation to each other. If now the arms be moved back, so as to cause a less convergence of the axes, the image will appear to increase in magnitude until the arms are in a straight line and the optic axes are parallel; and, on the other hand, if the arms be moved forwards, so as to form a less angle, the optic axes will converge more, and the image will appear gradually smaller. In this manner, while the retinal magnitude remains the same, the perceived magnitude of the binocular object varies through a very considerable range.

The instrument being again adjusted so that the image shall be seen of its natural size; on sliding the pictures nearer the mirrors its perceived magnitude will be augmented, and on sliding them from the mirrors it will appear diminished in size. During these variations of magnitude the inclination of the optic axes remains the same.

The perceived magnitude of an object, therefore, diminishes as the inclination of the axes becomes greater, while the distance remains the same; and it increases, when the inclination of the axes remains the same, while the distance diminishes. When both these conditions vary inversely, as they do in ordinary vision when the distance of an object changes, the perceived magnitude remains the same*.

Before I proceed further it will be proper to explain the meaning of some of the terms I employ. I call the magnitude of the object itself, the real or objective mag-

* Several cases of the alteration of the perceived magnitude of objects are mentioned by Dr. R. Smith (Complete System of Opticks, 1738, vol. ii. p. 388, and rem. 526 and 532); and Dr. R. Darwin (Philosophical Transactions, vol. lxxvi. p. 313) observed that when an ocular spectrum was impressed on both eyes it appeared magnified when they were directed to a wall at a considerable distance. The facts noticed by these authors are satisfactorily explained by the above considerations.

nitude; the magnitude of the picture on the retina, the retinal magnitude; and the magnitude we estimate the object to be from its retinal magnitude and the inclination of the optic axes conjointly, I name the perceived magnitude. I do not use the term apparent magnitude, because, according to its ordinary acceptation, it sometimes means what I call retinal, and at other times what I name perceived magnitude.

We have seen in what manner our perception of magnitude is modified by the new associations which this instrument enables us to form; let us now examine how our perception of distance is affected by them. If we continue to observe the binocular picture whilst it apparently increases or decreases, in consequence of the inclination of the optic axes varying while the magnitude of the impressions on the retinæ remains the same, it does not appear either to approach or to recede; and yet if we attentively regard it in any fixed position, it is perceived to be at a different distance. On the other hand, if we continue to regard the binocular picture, enlarging and diminishing in consequence of the change of retinal magnitude while the convergence of the axes remains the same, we perceive it to approach or recede in the most evident manner; but on fixing the attention to it, when it is stationary, at any instant, it appears to be at the same distance at one time as it is at another.

Convergence of the optic axes therefore suggests fixed distance to the mind; variation of retinal magnitude suggests change of distance. We may, as I have above shown, perceive an object approach or recede without appearing to change its distance, and an object to be at a different distance, without appearing to approach or recede; these paradoxical effects render it difficult, until the phenomena are well apprehended, to know, or to express, what we actually do perceive.

It is the prevalent opinion that the sensation which accompanies the inclination of the optic axes immediately suggests distance, and that the perceived magnitude of an object is a judgement arising from our consciousness of its distance and of the magnitude of its picture on the retina. From the experiments I have brought forward, it rather appears to me that what the sensation which is connected with the convergence of the axes immediately suggests is a correction of the retinal magnitude to make it agree with the real magnitude of the object, and that distance, instead of being a simple perception, is a judgement arising from a comparison of the retinal and perceived magnitudes. However this may be, unless other signs accompany this sensation the notion of distance we thence derive is uncertain and obscure, whereas the perception of the change of magnitude it occasions is obvious and unmistakeable.

To see, in their full extent, the variations of magnitude exhibited by the instrument I have described, it is necessary to attend to the following observations.

As the inclination of the optic axes corresponding to a different distance is habitually, under ordinary circumstances, accompanied with the particular adaptation of the eyes required for distinct vision at that distance, it is difficult to disassociate these two conditions so as to see with equal distinctness the binocular picture when

the optic axes are parallel, and when they converge greatly, although the pictures remain, in both cases, at the same distance from the eyes. The adaptation is, therefore, not entirely dependent on the divergence of the rays of light which proceed from the object regarded, but also, in some degree, on the inclination of the optic axes. I have acquired by practice considerable power of adjustment, or rather disadjustment, of the eyes, and can, without having recourse to artificial means, see the binocular picture distinctly when its perceived magnitude is widely different. Those to whom such an effort is painful may employ short-sighted spectacles to see the binocular picture when the eyes converge within the limit of distinct vision for the distance at which the pictures are placed; and long-sighted spectacles when the eyes converge beyond that limit, or become parallel.

There is a means of avoiding to a very considerable extent the influence of the adjustment of the eyes, and thereby enabling the pictures to be seen distinctly within the entire range of the inclination of the optic axes. This is by looking at the reflected images in the mirrors through two very minute apertures, not larger than fine pin-holes, placed near each eye, and illuminating the pictures by a very strong light; sunshine in the middle of the day answers the purpose very well. By this expedient the divergence of the rays of light is greatly diminished, and the adaptation of the eyes does not materially influence the result.

§ 18.

Leaving this subject, I will now revert to the stereoscope and its effects.

Since 1838 numerous modifications of the stereoscope have occurred to me, and several ingenious arrangements have also been proposed by Sir David Brewster and Prof. Dove; but there is no form of the instrument which has so many advantages for investigating the phenomena of binocular vision as the original reflecting stereoscope. Pictures of any size may be placed in it, and it admits of every kind of adjustment.

I have constructed a very portable reflecting stereoscope which is represented at fig. 3. The sides fold over the mirrors, and the mirrors then fold into a box, which is not larger than 6 inches in any of its dimensions. To avoid the second feeble reflection from the anterior surface of the silvered glass, which has a bad effect when the attention is attracted to it, I have sometimes employed reflecting prisms. The reflecting surfaces of the prisms should be silvered in order to obviate the unequal brightness of the field of view on each side of the limit of total reflection; and as it would be too costly to employ very large prisms, they should have an adjustment to accommodate their distance to the width between the eyes of the observer.

I have, for many years past, employed also another means to occasion, without any straining of the eyes, the coincidence of the pictures so that the image in relief shall appear of the same magnitude and at the same distance as the object which they represent would do if it were itself directly regarded. In this apparatus, prisms

being employed to deflect the rays of light proceeding from the pictures, so as to make them appear to occupy the same place, I have called it the refracting stereoscope.

It is represented by fig. 4. It consists of a base 6 inches long and 4 inches broad, upon which stands an upright partition, 5 inches high, dividing it equally; this partition is capable of extension by means of a slide to double the length, and carries at its upper extremity a board placed parallel to the base, and of the same dimensions. In this upper board there are two apertures an inch square, one on each side of the partition, the centres of which are $2\frac{1}{2}$ inches from each other; in these apertures are fixed a pair of glass prisms having their faces inclined 15°, and their refractive angles turned towards each other. The stereoscope pictures are to be placed on the base, and their centres ought not to exceed the distance of $2\frac{1}{2}$ inches.

A pair of plate-glass prisms, their faces making with each other an angle of 12°, will bring two pictures, the corresponding points of which are $2\frac{1}{2}$ inches apart, to coincidence at a distance of 12 inches, and a pair with an angle of 15° will occasion coincidence at 8 inches.

The refracting stereoscope has the advantage of portability, but it is limited to pictures of small dimensions. It is well suited for Daguerreotypes, which are usually of small size, and, on account of the nature of their reflecting surface, must be viewed in a particular direction with respect to the light which falls upon them; whereas in the reflecting stereoscope it is somewhat difficult to render the two Daguerreotypes equally visible. For drawings and Talbotypes it however offers no advantages, though it is equally well suited for them when their dimensions are small.

Stereoscopic drawings afford a means of illustrating works with figures of three dimensions, instead of with mere plane representations. Works on crystallography, solid geometry, spherical trigonometry, architecture, machinery, &c., might be thus rendered more instructive, from the perfect counterpart of the solid figure seen from a single point of view being represented, instead of merely one of its plane projections. For this purpose the corresponding binocular figures must be engraved in parallel vertical columns, and their coalescence may be effected by viewing them through a pair of prisms, similar to those employed in the refracting stereoscope, placed in a frame at the proper distance from each other. If the engravings should be less than $2\frac{1}{2}$ inches apart, the prisms may be dispensed with by persons who have command over the adaptation of their eyes, particularly if they be short-sighted.

§ 19.

At the date of the publication of my experiments on binocular vision, the brilliant photographic discoveries of Talbot, Niepce and Daguerre, had not been announced to the world. To illustrate the phenomena of the stereoscope I could therefore, at that time, only employ drawings made by the hands of an artist. Mere outline figures, or even shaded perspective drawings of simple objects, do not present much

difficulty; but it is evidently impossible for the most accurate and accomplished artist to delineate, by the sole aid of his eye, the two projections necessary to form the stereoscopic relief of objects as they exist in nature with their delicate differences of outline, light and shade. What the hand of the artist was unable to accomplish, the chemical action of light, directed by the camera, has enabled us to effect.

It was at the beginning of 1839, about six months after the appearance of my memoir in the Philosophical Transactions, that the photographic art became known, and soon after, at my request, Mr. Talbot, the inventor, and Mr. Collen (one of the first cultivators of the art) obligingly prepared for me stereoscopic Talbotypes of full-sized statues, buildings, and even portraits of living persons. M. Quetelet, to whom I communicated this application and sent specimens, made mention of it in the Bulletins of the Brussels Academy of October 1841. To M. Fizeau and M. Claudet I was indebted for the first Daguerreotypes executed for the stereoscope. The beautiful stereoscopic representations of statuary, architecture, machinery, natural history specimens, portraits of living persons, single and in groups, &c., which have recently been produced by M. Soleil and M. Claudet, are now too well known to the public to need more than a slight reference to them.

With respect to the means of preparing the binocular photographs (and in this general term I include both Talbotypes and Daguerreotypes), little requires to be said beyond a few directions as to the proper positions in which it is necessary to place the camera in order to obtain the two required projections.

We will suppose that the binocular pictures are required to be seen in the stereoscope at a distance of 8 inches before the eyes, in which case the convergence of the optic axes is about 18°. To obtain the proper projections for this distance, the camera must be placed, with its lens accurately directed towards the object, successively in two points of the circumference of a circle of which the object is the centre, and the points at which the camera is so placed must have the angular distance of 18° from each other, exactly that of the optic axes in the stereoscope. The distance of the camera from the object may be taken arbitrarily, for, so long as the same angle is employed, whatever that distance may be, the pictures will exhibit in the stereoscope the same relief, and be seen at the same distance of 8 inches, only the magnitude of the picture will appear different. Miniature stereoscopic representations of buildings and full-sized statues are therefore obtained merely by taking the two projections of the object from a considerable distance, but at the same angle as if the object were only 8 inches distant, that is, at an angle of 18°.

To produce the best effect, it is necessary that the pictures be so placed in the stereoscope that each eye shall see its respective picture at the proper point of sight: if this condition be not attended to, the binocular perspective will be incorrect.

For obtaining binocular photographic portraits, it has been found advantageous to employ, simultaneously, two cameras fixed at the proper angular positions.

I subjoin a Table of the inclinations of the optic axes which correspond to different

distances; it also shows the angular positions of the camera required to obtain binocular pictures which shall appear at a given distance in the stereoscope in their true relief.

Inclination of the optic axes	2°	4°	6°	8°	10°	12°	14°	16°	18°	20°	22°	24°	26°	28°	30°
Distance in inches.....	71·5	35·7	23·8	17·8	13·2	11·8	10·1	8·8	7·8	7·0	6·4	5·8	5·4	5·0	4·6

The distance is equal to $\frac{a}{2}\operatorname{cotang}\frac{\theta}{2}$; a denoting the distance between the two eyes, and θ the inclination of the optic axes.

§ 20.

As the inclination of the optic axes diminishes by the removal of an object to which they are directed to a greater distance, not only does the magnitude of the pictures projected by it on the retinæ proportionately diminish, but the dissimilarity of the pictures becomes less. The difference of distance between any two points of each of the pictures will diminish until the projections become sensibly similar. Under the usual circumstances attending the vision of a solid object placed at a given distance, a particular inclination of the axes is invariably accompanied by a specific pair of dissimilar projections; and if the distance be changed, a different inclination of the axes is accompanied by another pair of projections; but, by means of the stereoscope, we have it within our power to associate these circumstances abnormally, and to cause any degree of inclination of the axes to coexist with any dissimilarity of the two pictures. To ascertain experimentally what takes place under these circumstances M. Claudet prepared for me a number of Daguerreotypes of the same bust, taken at a variety of different angles, so that I was enabled to place in the stereoscope two pictures taken at any angular distance from 2° to 18°, the former corresponding with a distance of about 6 feet, and the latter with a distance of about 8 inches. The effect of a pair of near projections seen with a distant convergence of the optic axes, is to give an undue elongation to lines joining two unequally distant points, so that all the features of a bust appear to be exaggerated in depth. The effect, on the contrary, of a pair of distant projections, seen with a *near* convergence of the axes, is to give an undue shortening to the same lines, so that the appearance of a bas-relief is obtained from the two projections of the bust. The apparent dimensions in breadth and height remain in both cases the same.

§ 21.

To reproduce the conditions of the binocular vision of a solid object as completely as possible by means of its two plane projections, it is necessary, as I have before stated, that the projections shall be such as correspond exactly with the inclination of the optic axes under which they are viewed. I have already shown in § 20 what takes place when this condition is not strictly observed, and I may add that the

mind is not unpleasantly affected by a considerable incongruity in this respect; on the contrary, the effect in many cases seems heightened by viewing the solid appearance, intended for a determinate degree of inclination of the axes, under an angle several degrees less; the reality is as it were exaggerated. When the optic axes are parallel, in strictness there should be no difference between the pictures presented to each eye, and in this case there would be no binocular relief; but I find that an excellent effect is produced when the axes are nearly parallel by pictures taken at an inclination of 7° or 8°, and even a difference of 16° or 17° has no decidedly bad effect.

This circumstance enables us to combine the ideal amplification arising from viewing pictures placed near the eyes under a small inclination, or even parallelism, of the optic axes mentioned in § 17, with the perception of solidity arising from the dissimilarity of the projections; for this purpose, the pictures in the refracting stereoscope, or their reflected images in the reflecting instrument, must be viewed through lenses the focal distance of which is equal to the distance between them and the pictures; the perceived magnitude of the binocular image will increase with the nearness of the pictures, and depends almost entirely on the disassociation of the retinal magnitude from its usually accompanying inclination of the optic axes, the actual magnifying power of the lenses having a very small influence.

The sole use of the lenses is to render the rays of light parallel, which it is necessary they should be for distinct vision when the optic axes are parallel. When the reflecting stereoscope is employed, this means of magnifying the effect is not of much utility, as pictures of any size may be adapted to that instrument. But in the case of the refracting stereoscope it may be advantageously made use of. By combining lenses with the refracting stereoscope, described in § 18, Daguerreotypes somewhat wider than the width between the eyes may be employed. Sir David Brewster has used, to effect the same purpose, semi-lenses with their edges directed towards each other, which serve at the same time to render the rays less convergent and slightly to displace the pictures towards each other. Two corresponding Daguerreotypes, each not exceeding in breadth the width between the eyes, being placed close to each other, and viewed with lenses of short focal distance, will even without the aid of the prisms give an apparently highly magnified binocular image in bold relief.

There is a peculiarity in such images worthy of remark; although the optic axes are parallel, or nearly so, the image does not appear to be referred to the distance we should, from this circumstance, suppose it to be, but it is perceived to be much nearer, and indeed more so, as the pictures are nearer the eyes, though the inclination of the optic axes remains the same, and should therefore suggest the same distance; it seems as if the dissimilarity of the projections, corresponding as they do to a nearer distance than that which would be suggested by the former circumstance alone, alters in some degree the perception of distance.

I recommend, as a convenient arrangement of a refracting stereoscope for viewing

Daguerreotypes of small dimensions, the instrument represented, fig. 4, shortened in its length from 8 inches to 5, and lenses of 5 inches focal distance placed before and close to the prisms.

§ 22.

I now proceed to another subject—to the consideration of those phenomena which I have termed Conversions of Relief.

In § 5 of my first memoir I noticed the remarkable circumstance, that when the drawing intended to be seen by the right eye is presented to the left eye in the stereoscope, and *vice versâ*, a totally different solid figure is perceived to that seen before the transposition. I called this the converse figure, and showed that it differs from the normal figure in the circumstance, that those points which appear the most distant in the latter, appear the nearest in the former.

The pictures being, in the first place, presented directly to their corresponding eyes, as in the refracting stereoscope, and exhibiting therefore the resultant image in its normal relief, the conversion of the relief may be effected in three different ways,—1st, by transposing the pictures from one eye to the other, as mentioned above; 2ndly, by reflecting the pictures, while they remain presented to the same eye, as in the reflecting stereoscope; and 3rdly, by inverting the position of the pictures without transposing them.

The following considerations will explain the cause of the conversion of relief in the preceding cases.

If two different objects, or parts of an object (fig. 5 *a*), have a greater lateral distance between them on the right-hand picture than that which they have on the left-hand picture, the optic axes must converge more to make the left-hand than to make the right-hand objects coincide, and the left-hand object will appear the nearest.

If the pictures be now transposed from one eye to the other (fig. 5 *a'*), the greatest distance will be between the corresponding points of the picture presented to the left eye; the optic axes must therefore converge less to make the left-hand objects coincide, and the right-hand object will appear the nearest.

If the pictures, remaining untransposed, be each separately reflected (fig. 5 *b*), the relative distances of the corresponding objects remain the same to each eye, and the left-hand object will still appear nearest; but in consequence of the lateral inversion of the objects in each picture by reflexion, that which was previously on the left will now be on the right, and therefore, the object which before appeared nearest, will now appear farthest.

When the pictures are turned upside down, still remaining untransposed (fig 5 *c*), the objects are reversed with respect to the right and left, in the same manner as they are when reflected, and the lateral distances between the objects remaining the same to each eye, precisely the same conversion of relief is produced as in the preceding case, except that the resultant image is inverted. The diagram (fig. 5) repre-

sents all the possible changes of the two binocular pictures; those marked N show the normal relief, and those marked C the converse relief.

But it may be asked why, if the reflection or inversion of the binocular pictures of an object gives rise to the mental idea of the converse relief, the same converse relief is not observed when the object itself is reflected in a mirror, or inverted. The reason is this; that in the former cases the projections to each eye are separately reflected or inverted, still remaining presented to the same eye, whereas, by the reflection or inversion of the object itself, not only are the projections reflected or inverted, but they are also transposed from one eye to the other; and these circumstances occurring simultaneously reproduce the normal relief.

Fig. 6 will render this evident in the case of reflexion: A is the object, B its reflexion in the mirror CD; RB and LB are the directions in which the right and left eyes view the reflected image respectively, and *l*A and *r*A the directions in which the eyes would view the corresponding face of the object directly.

In the case of an inverted object, it is obvious that that projection which was before seen by the right eye must be seen by the left eye, and the contrary.

It is possible to make this normal or converse relief appear while one of the pictures remains constantly presented to the same eye. This result may be thus obtained. Having taken a photograph of the object, which should be one the converse of which has a meaning, take two others at the same angular distance (say 18°), one on the right side, the other on the left side of the original. Of the three pictures thus taken, if the middle one be presented to the right eye, and the left picture to the left eye, a normal relief will be seen; but if the right picture be presented to the left eye, the other remaining unchanged, a converse relief will be seen. In like manner, if the middle picture be presented to the left eye, and the right picture to the right eye, a normal relief will appear; but if the left picture be presented to the right eye, the converse relief will present itself. It must be observed, that the normal and converse reliefs, when the same picture remains presented to the same eye, belong to two different positions of the object.

§ 23.

Hitherto I have taken into consideration only those cases of the conversion of relief which are exhibited by binocular pictures in the stereoscope, when they are transposed, reflected or inverted; I shall now proceed to show how phenomena of the same kind may be elicited by regarding objects themselves, by means of an instrument adapted for the purpose. As this instrument conveys to the mind false perceptions of all external objects, I have called it the Pseudoscope. It is represented by fig. 7, and is thus constructed: two rectangular prisms of flint glass, the faces of which are 1·2 inch square, are placed in a frame with their hypothenuses parallel, and 2·1 inches from each other; each prism has a motion on an axis corresponding with the angle nearest the eyes, so that they may be adjusted that their bases may

have any inclination towards each other; and the frame itself is adjustable by a hinge at *a*, in order to bring the prisms nearer each other to suit the eyes of the observer.

The instrument being held to the eyes, and adjusted to an object, so that it shall appear single, each eye will see a reflected image of that projection of the object which would be seen by the same eye without the pseudoscope. This is exactly the contrary of what occurs when the eyes regard the reflected image of an object in a looking-glass; the left eye then sees the reflected image of the right-hand projection, and the right eye the reflected image of the left projection, as shown by fig. 6.

Plane mirrors cannot be substituted for the reflecting prisms, for this reason; the refraction of the rays of light at the incident and emergent surfaces of the prisms enables the reflexion of an object to be seen when the object is even behind the prolongation of the reflecting surface, as shown at fig. 8, and thus the reflected binocular image may be seen in the same place as the object itself, whereas the images cannot be made by means of plane mirrors thus to coincide.

When the pseudoscope is so adjusted as to see a near object while the optic axes are parallel, to view a more distant object with the same adjustment, the axes must converge, and the more so as the object is more distant; all nearer objects than that seen when the axes are parallel, will appear double, because the optic axes can never be simultaneously directed to them. If this instrument be so adjusted that very distant objects are seen single when the eyes are parallel, *all* nearer objects will appear double, because the optic axes can never converge to make their binocular images coincide. If the attention is required to be directed to an object at a particular distance, the best mode of viewing it with the pseudoscope is to adjust the instrument so that the object shall appear at the proper distance and of its natural size. In this case the more distant objects will appear nearer and smaller, and the nearer objects will appear more distant and larger.

In ordinary vision, whenever the distance of an object varies, the magnitude of the picture on the retina, and the degree of convergence of the optic axes, always maintain a constant relation to each other, both increasing or decreasing together; and the perceived magnitude, suggesting to the mind the real magnitude of the object, in consequence thereof remains the same. The instrument I described in § 17 shows what illusions arise when the usual relations of these elements of our perceptions are disturbed, by causing one to remain constant while the other varies. The pseudoscope exhibits the still more curious illusions, which result from combining these elements inversely, so that as an object becomes nearer, its larger picture on the retina is accompanied by a less convergence of the optic axes. With the pseudoscope we have a glance, as it were, into another visible world, in which external objects and our internal perceptions have no longer their habitual relation with each other.

I will now proceed to describe some of the illusions produced by the aid of this

instrument. Those which may be strictly designated conversions of relief, in which the illusive appearance has the same relation to that of the real object as a cast to a mould, or a mould to a cast, are very readily perceived. I must however remark, that it is necessary to illuminate the object equally, so as to allow no lights or shades to appear upon them, for their presence has a considerable influence on the judgement, and is one of the principal causes of the perception of the proper relief when a single eye is employed.

The inside of a tea-cup appears as a solid convex body; the effect is more striking if there are painted figures within the cup.

A china vase, ornamented with coloured flowers in relief, presents a very remarkable appearance; we apparently see a vertical section of the interior of the vase, with painted hollow impressions of the flowers.

A small terrestrial globe appears as a concave hemisphere; on turning it round on its axis, it was curious to see different portions of the spherical map appear and disappear in a manner that nothing in external nature can imitate.

A bust regarded in front becomes a deep hollow mask; the appearance when regarded in profile is equally striking.

A framed picture hanging against a wall, appears as if imbedded in a cavity made in the wall.

A medal, or the impression of a seal, is perfectly converted into a representation of the die from which it has been struck; and, on the other hand, the mould or die of a medal, or an engraved seal, becomes a *fac-simile* of the medal or raised impression. It will also be observed, that if the medal be placed on a flat surface, as a sheet of paper, it will appear sunk beneath the surface; and if it be placed in a hollow of the same size, it will appear to stand above the surface as much as it actually is below it.

These appearances are not always immediately perceived; and some much more readily present themselves than others. Those converse forms which have a meaning, and resemble real forms we have been accustomed to see, are those which are the most easily apprehended. Viewed with the pseudoscope, notwithstanding the inversion of the pictures on the retina, the natural appearance of the object continues to intrude itself, when sometimes suddenly, and at other times gradually, the converse occupies its place. The reason of this is, that the relief and distance of objects is not suggested to the mind solely by the binocular pictures and the convergence of the optic axes, but also by other signs, which are perceived by means of each eye singly; among which the most effective are the distributions of light and shade and the perspective forms which we have been accustomed to see accompany these appearances. One idea being therefore suggested to the mind by one set of signs, and another totally incompatible idea by another set, according as the mental attention is directed to the one and abstracted from the other, the normal

form or its converse is perceived. This mental attention is involuntary; no immediate effort of the will can call up one idea while the other continues to present itself, though the transition may be facilitated by intentionally removing some of the signs which suggest the preponderating idea; thus the converse form being perceived, closing either eye will most frequently cause an instant reversion to the normal form; and always, if the monocular signs of relief are sufficiently suggestive.

I know of nothing more wonderful, among the phenomena of perception, than the spontaneous successive occurrence of these two very different ideas in the mind, while all external circumstances remain precisely the same. Thus a small statuary group, an elegant and beautiful object, without any apparent cause becomes converted into another totally dissimilar object uncouth in appearance, and which gives rise to no agreeable emotions in the mind; yet in both cases all the sensations that intervene between objective reality and ideal conception continue unchanged.

The effects of the pseudoscope I have already mentioned, may be strictly called conversions of relief, because the illusive appearance is in each case the converse impression of the relief of the real object. If, however, the object consists of parts detached from and behind each other, the preceding term is inappropriate to denote the effects which result, but the more general expression conversion or inversion of distance may be employed to designate them. I proceed to call attention to a few such effects.

Skeleton figures of geometrical solids, as cubes, pyramids, &c., readily show their converse.

Two objects at different distances, being simultaneously regarded, the most remote will appear the nearest and the nearest the most remote.

An ivory foot rule, held immediately before the eyes a little inclined to the horizon with its remote end elevated, appears inclined in the opposite way, its nearer end elevated, and as if the observer were looking at its lower surface. Its form also undergoes a change. Since the nearest end, the retinal magnitude of which is the largest, appears farthest from the eyes, and the nearest end, the retinal magnitude of which is greatest, appears near the eyes, the rule will no longer be perceived to be rectangular, but trapezoidal. If the rule be placed horizontally, and it be regarded with the pseudoscope at an angle of 45°, it will appear with the form just described standing vertically.

Any object placed before the wall of a room will appear behind the wall, and as if an aperture of the proper dimensions had been made in the wall to allow it to be seen; if the object be illuminated by a candle, its shadow will appear as far before the object as in reality it is behind.

The appearance of a plant is very remarkable; as the branches which are farthest from the eye are perceived to be the nearest, those parts which are actually obscured by the branches before them, appear broken away and allow the parts apparently be-

hind them to be seen. A flowering shrub before a hedge appears to be transferred behind it; and a tree standing outside a window may be brought visibly within the room in which the observer is standing.

I have before observed that the transition from the normal to the converse perception is often gradual; I will give one instance of this as an illustration. The object was a page of medallions embossed on card-board, and the raised impressions were protected from injury by a thick piece of mill-board having apertures in it made to correspond to each medallion. The page was placed horizontally, illuminated by a candle placed beyond it, and looked at through the pseudoscope at an angle of 45°; for the first moment the page appeared as it would have done without the instrument; soon after the medallions appeared level with the upper surface, and the shadows on the upper parts of the circular apertures were converted into deep depressions as if cut out with a tool; they next, from horizontal, became vertical, each standing erect on the horizontal plane, and immediately afterwards the reliefs were all changed into hollows; finally, the page itself stood vertical, but with that change of form which I indicated in the case of the rule, the upper edge appearing much shorter than the lower edge: the series of changes being now complete, the final form remained constant as long as the object was regarded.

In endeavouring to analyse the phenomena of converse perception, it must be borne in mind that the transposition of distances has reference only to distances from the retinæ, not to absolute horizontal distances in space. Thus, if a straight ruler be held in the vertical plane perpendicular to the optic base, and also inclined 45° to the horizon so that its upper end shall be the most distant, when the eyes are directed horizontally towards it, the rule will appear exactly in the converse position. If the rule be now removed lower down in the same vertical plane, its inclination remaining unchanged, so that to look upon it the plane of the optic axes must be inclined 45°, it will appear unaltered in position, because its two pictures are parallel on the retinæ, and the optic axes would require the same convergence to make the upper and lower ends coalesce. The rule being removed still lower down, instead of its position being apparently reversed, it will appear to have a greater inclination on the same side than the object itself has. In the first case the more distant end is actually farthest from the eyes; in the second the near and remote ends are equally distant; and in the third the nearest end is most distant.

Attention to what I have just stated will explain many anomalous circumstances which occur when the eyes are differently directed towards the same object. It may also be necessary to remark, that the conversion of distance takes place only within those limits in which the optic axes sensibly converge, or the pictures projected on the retinæ are sensibly dissimilar. Beyond this range there is no mutual transposition of the apparent distances of objects with the pseudoscope; a distant view therefore appears unchanged.

Some very paradoxical results are obtained when objects in motion are viewed

through the pseudoscope. When an object approaches, the magnitude of its picture on the retinæ increases as in ordinary vision, but the inclination of the optic axes, instead of increasing, becomes less, as I have already explained. Now an enlargement of the picture on the retina invariably suggests approach, and a less convergence of the optic axes indicates that the object is at a greater distance; and we have thus two contradictory suggestions. Hence, if two objects be placed side by side at a certain distance before the eyes, and one of them be moved forwards, so as to vary its distance from the other, its continually enlarging picture on the retina makes it appear to come towards the eyes, as it actually does, while at the same time it appears at every step at a greater distance beyond the fixed object; from one suggestion the object appears to approach, from the other to have receded. I again observe that retinal magnitude does not itself suggest distance, but from its changes we infer changes of distance.

I have hitherto only described the pseudoscope constructed with two reflecting prisms. This is the most convenient apparatus for effecting the conversion of distance and relief that has occurred to me; but other means may be employed, which I will briefly mention.

1st. Two plane mirrors are placed together so as to form a very obtuse angle towards the eyes of the observer; immediately before them the object is to be placed at such distance that a reflected image shall appear in each mirror. The eyes being placed before and a little above the object, must be caused to converge to a point between the object and the mirrors; the right-hand image of the left eye will then unite with the left-hand image of the right eye, and the converse relief will be perceived. The disadvantages of this method are that only particular objects can be examined, and it requires a painful adaptation of the eye to distinct vision.

2ndly. Place between the object and each eye a lens of small focal distance, and adjust the distances of the object and the lenses so that distinct inverted images of the object shall be seen by each eye; on directing the eyes to the place of the object the two images will unite, and the converse relief be perceived. As the rays of light proceeding from the images have a greater divergence than those which would proceed from the point to which the optic axes are directed, long-sighted persons will see the binocular image more distinctly by wearing a pair of short-sighted spectacles. In this experiment the field of view is very small on account of the distance at which it is necessary to place the lenses from the eyes; but I have been enabled in this manner to see beautifully the converse relief of a small ivory bust and of other small objects, which however should be inverted in order to see them direct.

3rdly. The inverted images of the lenses, instead of being received immediately by the eyes as just described, may be thrown on a plate of ground glass as in the case of the ordinary camera-obscura, and may be then caused to unite by the means employed in any form of the refracting stereoscope.

§ 24.

The cases of the conversion of relief when the object is regarded with one eye only, some of which were known more than a century ago, were taken into consideration and endeavoured to be explained by me in § 11 of the first part of this memoir, and Sir David Brewster* has published some interesting and instructive observations on the same subject; I will therefore not revert to this matter here, but only to say that I have myself never observed the conversion of relief when looking with both eyes immediately on a solid object, and if it has been observed by others under such circumstances, I should be inclined to attribute the effect to an inequality in the impressions on the two eyes so that one only is attended to. But the plane shaded representation of a solid object, the relief of which is not very deep, may easily be made to appear at will either as the solid which it is intended to represent or as its converse, even when both eyes are employed. This effect is strikingly observed in the glyptographic engravings of medals of low relief, and depends entirely on whether the light is so placed that it would cast the same shadows on the real object as are represented in the picture, or that it would cast shadows in the opposite direction. In the former case the picture appears with the relief it was intended to suggest; in the latter with the converse relief. I have observed similar effects with Daguerreotypes of medallions and cameos, and with carefully shaded drawings of simple objects.

* Transactions of the Royal Society of Edinburgh, vol. xv. p. 365 and 657.

II. *On the Automatic Registration of Magnetometers, and Meteorological Instruments, by Photography.—No. IV.* By Charles Brooke, *M.B., F.R.S.*

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On the Automatic Temperature Compensation of the Force Magnetometers.

A PORTION of the funds liberally contributed by the Government for the advancement of science, and placed at the disposal of the President and Council of the Royal Society, having been entrusted to the author for the accomplishment of the above object on a plan which was submitted to the Astronomer Royal and Colonel Sabine in the spring of last year*, and by them considered feasible, he considers that he cannot better fulfil the obligation of reporting progress at the present period, than by laying before the Royal Society a description of the instruments now constructed.

So long as the results of the variations of magnetic force were deduced from eye-observation only, at the periods of which the temperature as well as the position of the magnets was recorded, a correction for the influence of change of temperature on the instruments themselves could be readily estimated and applied; but in deducing mean values from the photographic registers, especially those for intervals involving considerable changes of temperature, it is manifest that the greatest degree of accuracy cannot be attained, unless either the apparent values were individually corrected by means of a separate register of the thermometer enclosed in the box with the magnet, or the instrument possessed within itself an approximate automatic correction for the effects of change of temperature.

The object would not be unattainable by the former means, but the process would be both difficult and laborious; it therefore appeared more desirable to attempt its accomplishment by the latter. Referring therefore to the equation of equilibrium of the bifilar magnet, viz.

$$mX = W\frac{ab}{l}\sin\theta,$$

in which m is the magnetic moment of the bar, X the horizontal component of the earth's magnetic force, W the weight of the suspended bar and its appurtenances, l the length of the suspension skeins, a and b the upper and lower intervals of their centres, and θ the angle of torsion, it is evident that the object in view would be

* The author feels bound to express his belief that a somewhat similar plan of compensating the force magnetometers subsequently proposed by Mr. Brown at the last meeting of the British Association was entirely original.

accomplished, if by any mechanical agency either of the quantities W, a, or b could be *simultaneously* influenced by change of temperature, and *proportionally* to the altered value of m.

As it would be desirable that the correction should involve the small coefficient of the second power of the temperature, a very definite value of which has been determined*, it was at first proposed to act on W by means of a hollow glass globe attached to the suspension frame of the magnet, with a vertical tubular stem dipping into a cup of mercury. It is clear that as the elastic force of the air contained in the bulb is diminished by heat, a column of mercury in the stem would fall, and the diminution of the suspended weight thus occasioned, might by a due adjustment of the capacity of the bulb be rendered equivalent to the constant part of the loss of power in the bar, and would thus represent the coefficient of the first power of the temperature, while an equivalent to the coefficient of the second power might be obtained by a due adjustment of the *diameter* of the stem. It is evident that the smaller the diameter of the stem, or in other words, the longer the space occupied by the mercury depressed during a large interval of temperature, the greater would be the difference of the spaces corresponding to successive small intervals. But this arrangement would be liable to several small sources of error; first, an alteration of the suspended weight by hygrometric changes in the length of the suspension skein; secondly, a change in the bulk of the air in the bulb corresponding to barometric changes; and thirdly, a small and uncertain secular variation, from the evaporation of mercury in the cistern in which the stem would be immersed.

From these considerations, it has been deemed advisable to abandon the attempt to introduce in the compensation an equivalent for the small coefficient of t^2, and to rest satisfied with acting on a or b by means which would effect an equal change for equal intervals of temperature, and would therefore represent the coefficient of t only. Metallic expansion naturally suggested itself as the means of accomplishing this object; and b the lower interval of the skeins as the most appropriate point of application, inasmuch as the compensating apparatus would then be in close proximity with the magnet, and being isolated from the surrounding atmosphere by enclosure in the same case or cases, might be presumed to vary in temperature *slowly* and *simultaneously* with the magnet; and if the changes in temperature were not sufficiently simultaneous, the radiating capacity of the magnet might be increased by partially coating it with dead-black varnish, or diminished by gilding and burnishing it, to any required extent.

The compensating apparatus consists of a glass rod clamped at its middle point to the centre of magnet, the axes of the rod and bar being parallel: the free ends of the rod are enclosed in two zinc tubes, at the inner ends of which, where they nearly meet in the centre, and to their upper surface, two hooks are attached: two loops at the ends of the suspension skein are attached to these hooks, the skein passing over

* See a paper, No. III. by the Author in the Phil. Trans., Part I. for 1850.

a pulley at the point of suspension. Towards either end, the rod and tubes are connected by a moveable clamp, by which the two may be clamped together at any required distance from the centre. It is evident that by elevation of temperature the free ends of the zinc tubes will be approximated to each other by a quantity equal to the difference of the expansion of the lengths of zinc and glass that intervene between the sliding clamps and the free ends of the tubes, and consequently that a diminution to the same extent of the distance between the lower ends of the suspension skein will take place. The variation of the interval between the lower ends of the skein, corresponding with any given variation of temperature, may be made to bear any required ratio to the whole interval, first by a due adjustment of the upper and lower intervals of the skein, and secondly by varying the position of the sliding clamps, that is, of the acting lengths of the expanding tubes: the former may be considered as a coarse, the latter as a fine adjustment. The glass rod rests on rollers attached to the under surface of the tubes opposite to the hooks, in order that no jerking may be occasioned by the expansion or contraction of the zinc tubes. By these means the quantity b in the preceding formula may be made to vary by change of temperature, proportionably to the change of the quantity m with any required degree of exactness; so far, at least, as the variation of m is directly proportional to the variation of temperature.

In the adjustment of the instrument the following steps are necessary. First, let the temperature coefficient of the bar be determined by the method described in the Paper No. III. previously mentioned; secondly, let the lower interval of the skeins be taken, such that the ratio of the difference of linear expansion of the whole length of the tubes and glass rod (which for convenience is made the same length as the bar), between 32° F. and any given higher temperature, say 92° F., to the distance between the threads, may be considerably less than the whole correction for that interval of temperature; ample scope will thus be allowed for determining by experiment the requisite amount of compensation, as is the case in the adjustment of chronometers.

The pulley over which the skein passes at the upper point of suspension being made of brass, there will be an increase of the upper interval between the threads, with increase of temperature: in order to compensate for this, it will be necessary to take the acting lengths of the compensator somewhat greater than the calculated length, expressing the value of the temperature coefficient.

The instrument having been thus approximately adjusted, the magnet and its appurtenances are now to be enclosed in a rectangular jacketed zinc box. The water in the jacket may be raised to any required temperature, and the temperature maintained nearly constant for any required period, by heating a pipe connecting the inlet and outlet of the jacket by a jet or jets of gas.

A uniformity of the temperature of the jacket is obtained by the introduction of suitable diaphragms to ensure a complete circulation throughout its entire extent. A

photographic register of the variations of the instrument is now to be obtained by the means described in the two first papers of this series*, the temperature of a thermometer enclosed in the zinc box, and placed as near as possible to the magnet being simultaneously recorded at convenient intervals of time, corresponding to temperature changes of not more than two degrees. If the change of temperature is slow, that is, if a variation of 40° Fahr. is spread over a space of at least six or eight hours, the temperature of the mercury may fairly be assumed to be the same as that of the bar. If, in order to avoid the consumption of time, or for any other reason, it should be found desirable to observe during more rapid changes of temperature, some such means as those before mentioned must be employed to ensure a uniformity of radiating capacity in the magnet, the compensator, and the thermometer.

A photographic register of the bifilar variations during known changes of temperature having been thus obtained, should now be compared with that of another bifilar instrument of which the temperature has undergone comparatively little variation, as will be the case under ordinary circumstances: a comparison of these will readily show whether the instrument under trial is over- or under-compensated, and the requisite correction may be made by shifting the sliding clamps. A few repetitions of the same process will suffice to give the requisite accuracy of correction.

It may be remarked that the most satisfactory method would be to register simultaneously a carefully compensated instrument on the same sheet of paper; the means of doing this are not however at present in existence.

Compensation of the Balanced Magnetometer.

The horizontal displacement of the centre of gravity by metallic expansion is the most palpable means of effecting a compensation for the changes of temperature in this instrument; the only question is the precise method by which the numerical value of the temperature coefficients can be most nearly represented. The plan adopted has been that of attaching a small thermometer parallel to the axis of the magnet, and as nearly as possible in the same horizontal plane with the centre of gravity of the magnet and its appendages. The statical moment of the mercury displaced from the bulb of the thermometer by any given elevation of temperature, as $x°$ above 32° Fahr., may be represented by the same formula which expresses the temperature coefficients, namely,

$$cx+ex^2.$$

For let w be the weight of mercury contained in one degree of the tube, and let the tube be taken such that the distance from the centre of the bulb (which is presumed to be a symmetrical figure of revolution) to the point 32° Fahr. may be kc, and length of one degree $2ke$; then at any temperature $32°+x°$ the statical moment of the mercury displaced by a small change of temperature, dx, will be $w\,(kc+2kex)\,dx$,

* Published in the Philosophical Transactions, Part I., for 1847.

and consequently the statical moment of the mercury displaced between the temperatures of 32° and 32°+x° will be $(kcx+kex^2)\,w$; let now v be the weight which, placed at a unit of distance from the point of support, will represent the temperature correction for 1° above 32° Fahr.; it will only remain to obtain a bulb of such size that

$$kw=v;$$

we shall then clearly have the statical moment of the mercury displaced from the bulb by x° of elevation of temperature above 32° Fahr., and transferred to the vacant portion of the tube represented by

$$(cx+ex^2)\,v,$$

and consequently a correction in weight equivalent to the temperature coefficient will have been applied to the bar.

As the change of density of the thread of mercury in the stem has not been taken into the account, it will be better to place the point of the tube which corresponds to a distance $\frac{1}{2}kc$ below the point 32° opposite the knife-edge; the error from this cause will then be quite negligible.

The value of v is to be determined experimentally by observing the displacement of the register line occasioned by a small known weight placed on the bar at a known distance from the point of support, and comparing this with the scale coefficient obtained in the usual manner.

Owing to the short period of time that has elapsed since the completion of the instruments, and the difficulty of making accurate magnetic observations in a locality subject to the constant tremors and vibrations of a London thoroughfare, the constants have not been determined with a sufficient degree of accuracy for publication; when satisfactorily determined, they will be communicated to the Society in the form of a Supplement to this paper.

Description of the Plate.

Figs. 1 and 2. Plan and elevation of the bifilar compensator, half the actual size. *a a* the magnet, *b* the clamp which attaches the glass rod to the magnet. *c c* the zinc tubes enclosing the glass rod. *d d* the adjusting clamps, consisting of two parts; the outer encircles the zinc tube, the inner passes, and nearly fills the interval, between the tube and glass rod. They are capable of sliding for adjustment when the screws are loosened; when tightened, the rod and tubes are held together. *e e* screws for adjusting the distance between the hooks *h h*; these should be withdrawn, when the clamps *d* are fixed. *o o*, fig. 2, are the ends of the clamping pieces interposed between the tubes and the rod.

Fig. 3. The brass collar to which the hook is attached seen in section, full size. The glass rod *f* rests on a roller *g*, that there may be no jerking in the expansion and contraction of the tube; *i i* two screws for fixing the collar while the scale and temperature coefficients are determined; when these are tightened the clamps *d* and screws *e* should be relaxed.

Fig. 4. Plan of the balanced magnet compensator. *k k* the magnet. *l l* the thermometer tube held in two Ys by a spring *m* tightened by a screw. *n n* the clamp that attaches the magnet to the frame in which the agate knife-edges are fixed.

III. *Experimental Researches in Electricity.—Twenty-eighth Series.* By MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Ord. Boruss. Pour le Mérite, Eq., Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, Munich, Bruxelles, Vienna, Bologna, &c. &c.*

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§ 36. *On Lines of Magnetic Force; their definite character; and their distribution within a Magnet and through Space.*

3070. FROM my earliest experiments on the relation of electricity and magnetism (114. note), I have had to think and speak of lines of magnetic force as representations of the magnetic power; not merely in the points of quality and direction, but also in quantity. The necessity I was under of a more frequent use of the term in some recent researches (2149. &c.), has led me to believe that the time has arrived, when the idea conveyed by the phrase should be stated very clearly, and should also be carefully examined, that it may be ascertained how far it may be truly applied in representing magnetic conditions and phenomena; how far it may be useful in their elucidation; and, also, how far it may assist in leading the mind correctly on to further conceptions of the physical nature of the force, and the recognition of the possible effects, either new or old, which may be produced by it.

3071. A line of magnetic force may be defined as that line which is described by a very small magnetic needle, when it is so moved in either direction correspondent to its length, that the needle is constantly a tangent to the line of motion; or it is that line along which, if a transverse wire be moved in either direction, there is no tendency to the formation of any current in the wire, whilst if moved in any other direction there is such a tendency; or it is that line which coincides with the direction of the magnecrystallic axis of a crystal of bismuth, which is carried in either direction along it. The direction of these lines about and amongst magnets and electric currents, is easily represented and understood, in a general manner, by the ordinary use of iron filings.

3072. These lines have not merely a determinate direction, recognizable as above (3071.), but, because they are related to a polar or antithetical power, have opposite qualities or conditions in opposite directions; these qualities, which have to be distinguished and identified, are made manifest to us, either by the position of the ends of the magnetic needle, or by the direction of the current induced in the moving wire.

3073. A point equally important to the definition of these lines is, that they represent a determinate and unchanging amount of force. Though, therefore, their forms, as they exist between two or more centres or sources of magnetic power, may vary very greatly, and also the space through which they may be traced, yet the sum of power contained in any one section of a given portion of the lines is exactly equal to the sum of power in any other section of the same lines, however altered in form, or however convergent or divergent they may be at the second place. The experimental proof of this character of the lines will be given hereafter (3109. &c.).

3074. Now it appears to me that these lines may be employed with great advantage to represent the nature, condition, direction and comparative amount of the magnetic forces; and that in many cases they have, to the physical reasoner at least, a superiority over that method which represents the forces as concentrated in centres of action, such as the poles of magnets or needles; or some other methods, as, for instance, that which considers north or south magnetisms as fluids diffused over the ends or amongst the particles of a bar. No doubt, any of these methods which does not assume too much, will, with a faithful application, give true results; and so they all ought to give the same results as far as they can respectively be applied. But some may, by their very nature, be applicable to a far greater extent, and give far more varied results, than others. For just as either geometry or analysis may be employed to solve correctly a particular problem, though one has far more power and capability, generally speaking, than the other; or just as either the idea of the reflexion of images, or that of the reverberation of sounds may be used to represent certain physical forces and conditions; so may the idea of the attractions and repulsions of centres, or that of the disposition of magnetic fluids, or that of lines of force, be applied in the consideration of magnetic phenomena. It is the occasional and more frequent use of the latter which I at present wish to advocate.

3075. I desire to restrict the meaning of the term *line of force*, so that it shall imply no more than the condition of the force in any given place, as to strength and direction; and not to include (at present) any idea of the nature of the physical cause of the phenomena; or to be tied up with, or in any way dependent on, such an idea. Still, there is no impropriety in endeavouring to conceive the method in which the physical forces are either excited, or exist, or are transmitted; nor, when these by experiment and comparison are ascertained in any given degree, in representing them by any method which we adopt to represent the mere forces, provided no error is thereby introduced. On the contrary, when the natural truth and the conventional representation of it most closely agree, then are we most advanced in our knowledge. The emission and the ether theories present such cases in relation to light. The idea of a fluid or of two fluids is the same for electricity; and there the further idea of a current has been raised, which indeed has such hold on the mind as occasionally to embarrass the science as respects the true character of the physical agencies, and may be doing so, even now, to a degree which we at present little suspect. The

same is the case with the idea of a magnetic fluid or fluids, or with the assumption of magnetic centres of action of which the resultants are at the poles. How the magnetic force is transferred through bodies or through space we know not; whether the result is merely action at a distance, as in the case of gravity; or by some intermediate agency, as in the cases of light, heat, the electric current, and (as I believe) static electric action. The idea of magnetic fluids, as applied by some, or of magnetic centres of action, does not include that of the latter kind of transmission, but the idea of lines of force does. Nevertheless, because a particular method of representing the forces does not include such a mode of transmission, the latter is not therefore disproved; and that method of representation which harmonizes with it may be the most true to nature. The general conclusion of philosophers seems to be, that such cases are by far the most numerous, and for my own part, considering the relation of a vacuum to the magnetic force and the general character of magnetic phenomena external to the magnet, I am more inclined to the notion that in the transmission of the force there is such an action, external to the magnet, than that the effects are merely attraction and repulsion at a distance. Such an action may be a function of the ether; for it is not at all unlikely that, if there be an ether, it should have other uses than simply the conveyance of radiations (2591. 2787.). Perhaps when we are more clearly instructed in this matter, we shall see the source of the contradictions which are supposed to exist between the results of COULOMB, HARRIS and other philosophers, and find that they are not contradictions in reality, but mere differences in degree, dependent upon partial or imperfect views of the phenomena and their causes.

3076. Lines of magnetic force may be recognized, either by their action on a magnetic needle, or on a conducting body moving across them. Each of these actions may be employed also to indicate, either the direction of the line, or the force exerted at any given point in it; and this they do with advantages for the one method or the other under particular circumstances. The actions are however very different in their nature. The needle shows its results by attractions and repulsions; the moving conductor or wire shows it by the production of a current of electricity. The latter is an effect entirely unlike that produced on the needle, and due to a different action of the forces; so that it gives a view and a result of properties of the lines of force, such as the attractions and repulsions of the needle could never show. For this and other reasons I propose to develope and apply the method by a moving conductor on the present occasion.

3077. The general principles of the development of an electric current in a wire moving under the influence of magnetic forces, were given on a former occasion, in the First and Second Series of these Researches (36. &c.); it will therefore be unnecessary to do more than to call attention, at this time, to the special character of its indi-

cations as compared to those of a magnetic needle, and to show how it becomes a peculiar and important addition to it, in the illustration of magnetic action.

3078. The moving wire produces its greatest effect and indication, not when passing from stronger to weaker places, or the reverse, but when moving in places of equal action, *i. e.* transversely across the lines of force (217.).

3079. It determines the direction of the polarity by an effect entirely independent of pointing or such like results of attraction or repulsion; *i. e.* by the direction of the electric current produced in it during the motion*.

3080. The principle can be applied to the examination of the forces *within* numerous solid bodies, as the metals, as well as outside in the air. It is not often embarrassed by the difference of the surrounding media, and can be used in fluids, gases or a vacuum with equal facility. Hence it can penetrate and be employed where the needle is forbidden; and in other cases where the needle might be resorted to, though greatly embarrassed by the media around it, the moving wire may be used with an immediate result (3142.).

3081. The method can even be applied with equal facility to the interior of a magnet (3116.), a place utterly inaccessible to the magnetic needle.

3082. The moving wire can be made to sum up or give the resultant at once of the magnetic action at many different places, *i. e.* the action due to an area or section of the lines of force, and so supply experimental comparisons which the needle could not give, except with very great labour, and then imperfectly. Whether the wire moves directly or obliquely across the lines of force, in one direction or another, it sums up, with the same accuracy in principle, the amount of the forces represented by the lines it has crossed (3113.).

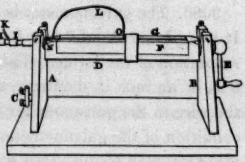
3083. So a moving wire may be accepted as a correct philosophical indication of the presence of magnetic force. Illustrations of the capabilities already referred to, will arise and be pointed out in the present paper; and though its sensibility does not as yet approach to that of the magnetic needle, still, there is no doubt that it may be very greatly increased. The diversity of its possible arrangements, and the great advantage of that diversity, is already very manifest to myself. Though both it and the needle depend for their results upon essential characters and qualities of the magnetic force, yet those which are influential, and, therefore indicated, in the one case, are very different from those which are active in the other; I mean, as far as we have been able as yet to refer directly the effects to essential characters: and this difference may, hereafter, enable the wire to a give a new insight into the nature of the magnetic force; and so it may, finally, bear upon inquiries, such as whether magnetic polarity is axial or dependent upon transverse lateral conditions; whether

* A natural standard of this polarity may be obtained, by referring to the lines of force of the earth, in the northern hemisphere, thus:—if a person with arms extended move forward in these latitudes, then the direction of the electric current, which would tend to be produced in a wire represented by the arms, would be from the right-hand through the arm and body towards the left.

the transmission of the force is after the manner of a vibration or current, or simply action at a distance; and the many other questions that arise in the minds of those who are pursuing this branch of knowledge.

3084. I will proceed to take the case of a simple bar magnet, employing it in illustration of what has been said respecting the lines of force and the moving conductor, and also for the purpose of ascertaining how these lines of force are disposed, both without and within the magnet itself, upon which they are dependent or to which they belong. For this purpose the following apparatus was employed. Let fig. 1 represent a wooden stand, of which the base is a board 17·5 inches in length, and 6 inches in breadth, and 0·8 of an inch in thickness: these dimensions will serve as a scale for the other parts. A B are two wooden uprights; D is an axis of wood having two long depressions cut into it, for the purpose of carrying the two bar magnets F and G. The wood is not cut away quite across the axis, but is left in the middle, so that the magnets are about $\frac{1}{15}$th of an inch apart. From O towards the supports A, it is removed, however, as low down as the axis of revolution, so as to form a notch between the two magnets when they are in their places; and by further removal of the wood, this notch is continued on to the end of the axis at P. This notch, or opening, is intended to receive a wire, which can be carried down the axis of rotation, and then passing out between the two magnets, anywhere between O and N, can be returned towards the end P on the outside. The magnets are so placed, that the central line of their compound system coincides with the axis of rotation; E being a handle by which rotation, when required, is given. H and I are two copper rings, slipping tightly on to the axis, by which communication is to be made between a wire adjusted so as to revolve with the magnets, and the fixed ends of wires proceeding from a galvanometer. Thus, let L represent a covered wire; which being led along the bottom of the notch in the axis of the apparatus, and passing out at the equatorial parts of the magnets, returns into the notch again near N, and terminates at P. When the form of the wire loop is determined and given to it, then a little piece of soft wood is placed between the wires in the notch at K, of such thickness, that when the ring I is put into its place, it shall press upon the upper wire, the piece of wood, and the lower wire, and keep all tightly fixed together, and at the same time leave the two wires effectually separated. The second ring, H, is then put into its place on the axis, and the introduction of a small wedge of wood, at the end of the axis, serves to press the end P into close and perfect contact with the ring H, and keep all in order. So the wire is free to revolve with the magnets, and the rings H and I are its virtual terminations.

Fig. 1.

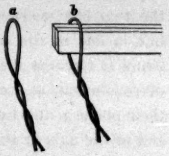


Two clips, as at C, hold the ends of the galvanometer wire (also of copper); and the latter are made to press against the rings by their elasticity, and give an effectual contact bearing, which generates no current, either by difference of nature or by friction, during the revolution of the axis.

3085. The two magnets are bars, each 12 inches long, 1 inch broad, and 0·4 of an inch thick. They weigh each 19 ounces, and are of such a strength as to lift each other end to end and no more. When the two are adjusted in their place, it is with the similar poles together, so that they shall act as one magnet, with a division down the middle: they are retained in their place by tying, or, at times, by a ring of copper which slips tightly over them and the axis.

3086. The galvanometer is a very delicate instrument made by RUHMKORFF (2651.). It was placed about 6 feet from the magnet apparatus, and was not affected by any revolution of the latter. The wires, connecting it with the magnets, were of copper, 0·04 of an inch in diameter, and in their whole length about 25 feet. The length of the wire in the galvanometer I do not know; its diameter was $\frac{1}{155}$th of an inch. The condition of the galvanometer, wires, and magnets, was such, that when the bend of the wires was formed into a loop, and that carried once over the pole of the united magnets, as from *a* to *b*, fig. 2, the galvanometer needle was deflected two degrees or more. The vibration of the needle was slow, and it was easy therefore to reiterate this action five or six times, or oftener, breaking and making contact with the galvanometer at right intervals, so as to combine the effect of like induced currents; and then a deflection of 10° or 15° on either side of zero could be readily obtained. The arrangement, therefore, was sufficiently sensible for first experiments; and though the resistance opposed by the thin long galvanometer wire to feeble currents was considerable, yet it would always be the same, and would not interfere with results, where the final effect was equal to 0°, nor in those where the consequences were shown, not by absolute measurement, but by comparative differences.

Fig. 2.

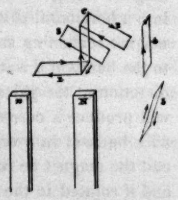


3087. The first practical result produced by the apparatus described, in respect of magneto-electric induction generally, is, that a piece of metal or conducting matter which moves across lines of magnetic force, has, or tends to have, a current of electricity produced in it. A more restricted and precise expression of the full effect is the following. If a continuous circuit of conducting matter be traced out, or conceived of, either in a solid or fluid mass of metal or conducting matter, or in wires or bars of metal arranged in non-conducting matter or space; which being moved, crosses lines of magnetic force, or being still, is by the translation of a magnet crossed by such lines of force; and further, if, by inequality of angular motion, or by contrary motion of different parts of the circuit, or by inequality of the motion in the same direction, one part crosses either more or fewer lines than the other; then a current will exist round it, due to the differential relation of the two or more inter-

secting parts during the time of the motion: the direction of which current will be determined (with lines having a given direction of polarity) by the direction of the intersection, combined with the relative amount of the intersection in the two or more efficient and determining (or intersecting) parts of the circuit.

3088. Thus, if fig. 3 represent a magnetic pole N, and over it a circuit, formed of metal, which may be of any shape, and which is at first in the position *c*; then if that circuit be moved in one direction into the position 1; or in the contrary direction into position 2; or by a double direction of motion into position 3; or by translation into position 4; or into position 5; or any position between the first and these or any resembling them; or, if the first position *c* being retained, the pole move to, or towards, the position *n*; then, an electric current will be produced in the circuit, having in every case the same direction, being that which is marked in the figure by arrows. Reverse motions will give currents in the reverse direction (256. &c.).

Fig. 3.

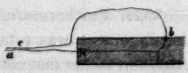


3089. The general principles of the production of electrical currents by magnetic induction have been formerly given (27. &c.)*, and the law of the direction of the current in relation to the lines of force, stated (114, 3079 note). But the full meaning of the above description can only be appreciated hereafter, when the experimental results, which supply a larger knowledge of the relations of the current to the *lines of force*, have been described.

3090. When *lines of force* are spoken of as crossing a conducting circuit (3087.), it must be considered as effected by the *translation* of a magnet. No mere rotation of a bar magnet on its axis, produces any induction effect on circuits exterior to it; for then, the conditions above described (3088.) are not fulfilled. The system of power about the magnet must not be considered as revolving with the magnet, any more than the rays of light which emanate from the sun are supposed to revolve with the sun. The magnet may even, in certain cases (3097.), be considered as revolving amongst its own forces, and producing a full electric effect, sensible at the galvanometer.

3091. In the first instance the wire was carried down the axis of the magnet to the middle distance, then led out at the equatorial part, and returned on the outside; fig. 4 will represent such a disposition. Supposing the magnet and wire to revolve once, it is evident that the wire *a* may be considered as passing in at the axis of the magnet, and returning from *b* across the lines of force external to the magnet, to the axis again at *c*; and that in one revolution,

Fig. 4.



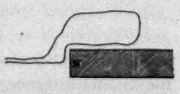
* Philosophical Transactions, 1832, page 131, &c.

the wire from *b* to *c* has intersected once, all the lines of force emanating from the N end of the magnet. In other words, whatever course the wire may take from *b* to *c*, the whole system of lines belonging to the magnet has been *once* crossed by the wire. In order to have a correct notion of the relation of the result, we will suppose a person standing at the handle E, fig. 1 (3084.), and looking along the magnets, the magnets being fixed, and the wire loop from *b* to *c* turned over toward the left-hand into a horizontal plane; then, if that loop be moved over towards the right-hand, the magnet remaining stationary, it will be equivalent to a *direct* revolution (according to the hands of a watch or clock) of 180°, and will produce a feeble current in a given direction at the galvanometer. If it be carried back 180° in the reverse direction, it will produce a corresponding current in the reverse direction to the former. If the wire be held in a vertical, or any other plane, so that it may be considered as fixed, and the magnet be rotated through half a revolution, it will also produce a current; and if rotated in the contrary direction, will produce a contrary current; but as to the *direction* of the currents, that produced by the *direct* revolution of the wire is the same as that produced by the *reverse* revolution of the magnet; and that produced by the *reverse* revolution of the wire is the same as that produced by the *direct* revolution of the magnet. A more precise reference of the direction of the current to the particular pole employed, and the direction of the revolution of the wire or magnet, is not at present necessary; but if required is obtained at once by reference to fig. 3 (3088.), or to the general law (114. 3079. note).

3092. The magnet and loop being rotated together in either direction, no trace of an electric current was produced. In this case the effect, if any, could be greatly exalted, because the rotation could be continued for 10, 20, or any number of revolutions without derangement, and it was easy to make thirty revolutions or more within the time of the swing of the galvanometer needle in one direction. It was also easy, if any effect were produced, to accumulate it upon the galvanometer by reversing the rotation at the due time. But no amount of revolution of the magnet and wire together could produce any effect.

3093. The loop was then taken out of the axis of the magnet, but attached to it by a piece of pasteboard, so that all should be fixed together and revolve with the same angular velocity, fig. 5; but whatever the shape or disposition of the loop, whether large or small, near or distant, open or shut, in one plane, or contorted into various planes; whatever the shape or condition, or place, provided it moved altogether with the magnet, no current was produced.

Fig. 5.

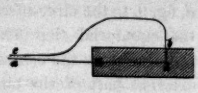


3094. Furthermore, when the loop was out of the magnets, and by expedients of arrangement, was retained immoveable, whilst the magnet revolved, no amount of rotation of the magnet (unaccompanied by translation of place) produced any degree of current through the loop.

3095. The loop of wire was then made of two parts; the portion *c*, fig. 6, on the

outside of the magnet, was fixed at *b*, and the portion *a*, being a separate piece, was carried along the axis until it came in contact with the former at *d*; the revolution of one part was thus permitted either with or without the other, yet preserving always metallic contact and a complete circuit for the induced current. In this case, when the external wire and the magnet were fixed, no current was produced by any amount of revolution of the wire *a* on its axis. Neither was any current produced when the magnet and wire, *c d*, were revolved together, whether the wire *a* revolved with them or not. When the magnet was revolved without the external part of *c d*, or the latter revolved without the magnet, then currents were produced as before (3091.).

Fig. 6.

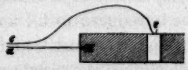


3096. The magnet was now included in the circuit, in the following manner. The wire *a*, fig. 7, was placed in metallic contact on both sides of the interval between the magnets at N (or the pole), and the part *c* was brought into contact with the centre at *d*. The result was in everything the same as when the wire *a* was continued up to *d*, *i. e.* no amount of revolution of the magnet and part *c* together could produce any electric current. When *c* was made to terminate at *e* or the equatorial part of the magnet, the result was precisely the same. Also, when *c* terminated at *e*, the part *a* of the wire was continued to the centre at *d*, and there the contact perfected, but the result was still the same. No difference, therefore, was produced, by the use between N and *d*, or *d* and *e*, of the parts of the magnet in place of an insulated copper wire, for the completion of the circuit in which the induced current was to travel. No rotation of the part *a* produced any effect, wherever it was made to terminate.

Fig. 7.

3097. In order to obtain the power of rotating the magnet without the external part of the wire, a copper ring was fixed round, and in contact with it at the equatorial part, and the wire *c*, fig. 8, made to bear by spring pressure against this ring, and also against the ring H on the axis, fig. 1 (3084.); the circuit was examined, and found complete. Now when the wire *c e* was fixed and the magnet rotated, a current was produced, and that to the same amount for the same number of revolutions, whether the part of the wire *a* terminated at N, or was continued on to the centre of the magnet, or was insulated from the magnet and continued up to the copper ring *e*. When the wire, by expedients, which though rough were sufficient, was made to revolve whilst the magnet was still, currents in the contrary direction were produced, in accordance with the effect before described (3091.); and the results when the wire and magnet rotated together (3092.), show that these are in amount exactly equal to the former. When the inner and the outer wires were both motionless, and the magnet only revolved, a current in the full proportion was produced, and that, whether the axial wire *a* made contact at the pole of the magnet or in the centre.

Fig. 8.

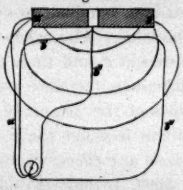


3098. Another arrangement of the magnet and wires was of the following kind. A radial insulated wire was fixed in the middle of the magnets, from the centre *d*, fig. 9, to the circumference *b*, being connected there with the equatorial ring (3097.); an axial wire touched this radial wire at the centre and passed out at the pole; the external part of the circuit, pressing on the ring at the equator, proceeded on the outside over the pole to form the communication as before. In the case where the magnet was revolved without the axial and the external wire, the full and proper current was produced; the small wire, *d b*, being, however, the only part in which this current could be generated by the motion; for it replaced, under these circumstances, the body of the magnet employed on the former occasion (3097.).

Fig. 9.

3099. The external part of the wire, instead of being carried back over that pole of the magnet at which the axial wire entered, was continued away over the other pole, and so round by a long circuit to the galvanometer; still the revolution of the magnet, under any of the described circumstances, produced exactly the same results as before. It will be evident by inspection of fig. 10, that, however the wires are carried away, the general result will, according to the assumed principles of action, be the same; for if *a* be the axial wire, and *b'*, *b''*, *b'''* the equatorial wire, represented in three different positions, whatever magnetic lines of force pass across the latter wire in one position, will also pass across it in the other, or in any other position which can be given to it. The distance of the wire at the place of intersection with the lines of force, has been shown, by the experiments (3093.), to be unimportant.

Fig. 10.



3100. Whilst considering the condition of the forces of a magnet, it may be admitted, that the two magnets used in the experimental investigations described, act truly as one central magnet. We have only to conceive smaller similar magnets to be introduced to fill up the narrow space not occupied by the wire, and then the complete magnet would be realized:—or it may be viewed as a magnet once perfect, which has had certain parts removed; and we know that neither of these changes would disturb the general disposition of the forces. In and around the bar magnet the forces are distributed in the simplest and most regular manner. Supposing the bar removed from other magnetic influences, then its power must be considered as extending to any distance, according to the recognized law; but, adopting the representative idea of *lines of force* (3074.), any wire or line proceeding from a point in the magnetic equator of the bar, over one of the poles, so as to pass through the magnetic axis, and so on to a point on the opposite side of the magnetic equator, must intersect *all* the lines in the plane through which it passes, whether its course be over the one pole or the other. So also a wire proceeding from the end of the magnet at the magnetic axis, to a point at the magnetic equator, must intersect

curves equal to half those of a great plane, however small or great the length of the wire may be; and though by its tortuous course it may pass out of one plane into another on its way to the equator.

3101. Further, if such a wire as that last described be revolved once round the end of the magnet to which it is related, a slipping contact at the equator being permitted for the purpose, it will intersect *all* the lines of force during the revolution; and that, whether the polar contact is absolutely coincident with the magnetic axis, or is anywhere else at the end of the bar, provided it remain for the time unchanged. All this is true, though the magnet may be subject, by induction at a distance, to other magnets or bodies, and may be exerting part of its force on them, so as to make the distribution of its power very irregular as compared to the case of the independent bar (3084.), or may have an irregular or contorted shape, even up to the horseshoe form. It is evident, indeed, that if a wire have one of its ends applied to *any* point on the surface of a magnet, and the other end to a point in the magnetic equator, and the latter be slipped once round the magnetic equator, and the loop of wire be made to pass over either pole, so as at last to resume its first position, it will in the course of its journey have intersected *once* every line of force belonging to the magnet.

3102. A wire from pole to pole which passes close to the equator, of course intersects half the external lines of force in a great plane, twice, in opposite directions as regards the polarity; and, therefore, when revolved round the magnet, has no electric current induced in it. If it do not touch at the equator, still, whatever lines it intersects, are twice intersected, and so the same equilibrium is preserved. If the magnet rotate under the wire, it acts the part of the central rotating wire already referred to (3095.); or if any course for the electric current other than a right line is assumed in it, that course is subject to the law of neutrality above stated, as will be seen by reference to the internal condition of the magnet itself (3117.). Hence the reason why no currents are produced, under any circumstances of motion, by the application of such conducting circuits to the magnet. I may further observe, in reference to the intersection of the lines of force, that if a wire ring, a little larger in diameter than the magnet, be held edgeways at one of the poles, so that the lines of force there shall be in its plane, and be then turned 90° and carried over the pole to the equator (3088.), it will intersect *once* all the lines of the magnet, except the very few which will remain unintersected at the equator.

3103. Whilst endeavouring to establish experimentally the definite amount of the power represented by the *lines of force*, it is necessary to take certain precautions, or the results will be in error. For instance, ten revolutions of the wire about the magnet, or of the magnet within the fixed wire (3097.), ought to give a constant deflection at the galvanometer, and yet without any change in the position of the wire the results may at different times differ very much from each other; being at one time 9°, and at another only 4° or 5°. I found this to be due to difference of velocity within certain limits, and to be explained and guarded against as follows.

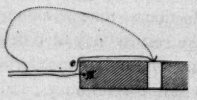
3104. If a wire move across lines of force slowly, a feeble electric current is produced in it, continuing for the time of the motion; if it move across the same lines quickly, a stronger current is produced for a shorter time. The effect of the current which deflects a galvanometer needle, is opposed by the action of the earth, which tends to return the needle to zero. A continuous weak current, therefore, cannot deflect it so far as a continuous stronger current. If the currents be limited in duration, the same effect will occur unless the time of the swing of the needle to one side be not considerably more than the time of either of the currents. If the time of the needle-swing be ten, and the time of ten quick rotations be six, then all the effect of the induced current is exerted in swinging the needle; but if the time of ten slow rotations be twelve or fifteen, then part of the current produced is not recognized by the extent of the vibration, but only by its holding the needle out awhile, at the extremity of a smaller arc of declination. Therefore, when quick and slow velocity was compared, and, indeed, in every case of comparative rotations of the wire and magnet, only that number of rotations was taken which could be well included within the time of the needle's journey to one side; when the needle, therefore, was seen to travel on to its extreme distance after the rotation and the inducing current had ceased. If the needle began to return the instant the motion was over, such an experiment was rejected for purposes of comparison. When these precautions were attended to, and velocities of revolution taken, which occupied times from one-third to three-fourths of that required for the swing of the needle, then the same number of revolutions (ten) gave the same amount of deflection, namely 9°·5, with my apparatus, though the time of revolution varied as 1 : 2, or even in a higher degree.

3105. Another cause of difference produced by varying velocity, is the diminution of the action of the current on the needle, as the angle which the latter forms with the convolutions of the coil increases. Hence a constant current produces more effect on the deflection of the needle for the first moments of time than afterwards. This effect, however, was scarcely sensible for swinging deflections of 9° or 10°, produced by currents which were over before the needle had moved through 4° or 5°.

3106. It has already been shown, that it is a matter of indifference whether the wire revolve in one direction or the magnet in the other (3091.); and this is still further proved by the cases where the magnet and the wire revolve together (3092.); for then the currents which tend to form are exactly equal and opposed to each other, whatever the position of the wire may be. As the immobility of the needle is a point more easily ascertained than the extent of an arc, indicated only for a moment, and as the rotations of the magnet and wire conjointly can be made rapid and continuous, the proof in such cases is very satisfactory.

3107. Proceeding to experiment upon the effect of the *distance* of the wire c, fig. 11, from the magnet, the wire was made to vary, so that sometimes it was not more than 8 inches long (being of copper and 0·04 of an inch in diameter), and only half an inch from the magnet, whilst at

Fig. 11.



other times it was 6 or 8 feet long and extended to a great distance. The deflection due to ten revolutions of the magnet was observed, and the average of several observations, for each position of the wire, taken: these were very close (with the precautions before described) for the same position; and the averages for different positions agreed perfectly together, being 9°·5. I endeavoured to repeat these experiments on distance by moving the wire and preserving the magnet stationary in the manner before described (3091.); they were not so striking because time would only allow of smaller deflections being obtained (3104.), but the same number of journeys through an arc of 180° gave the same deflection at the galvanometer, whether the course of the wire was close to the magnet or far off; and the deflection agreed with those obtained when the magnet was rotating and the wire at rest.

3108. As to *velocity* of motion; when the magnet was rotating and the wire placed at *different distances*, then ten revolutions of the magnet produced the same deflection of the needle, whether the motion was *quicker* or *slower*, and whatever the distance of the wire, provided the precautions before described were attended to (3104.). That the same would be true if the wire were moving and the magnet still, is shown by this; that whatever the velocity with which the wire and magnet revolve together, and whatever their distance apart, they exactly neutralize and equal each other (3096.).

3109. From these results the following conclusions may be drawn. The *amount* of magnetic force, as shown by its effect in evolving electric currents, is determinate for the same lines of force, whatever the distance of the point or plane, at which their power is exerted, is from the magnet. Or it is the same in any two, or more, sections of the same lines of force, whatever their form or their distance from the seat of the power may be. This is shown by the results with the magnet and the wire, when both are in the circuit (3108.); and also by the wire loop revolving with the magnet (3092.); where the tendencies of currents to form in the two parts oppose and exactly neutralize or compensate each other.

3110. In the latter case very varying sections outside of the magnet may be compared to each other; thus, the wire may be conceived of as passing (or be actually formed so as to intersect) lines of force near the pole, and then, being continued *along* a line of force until over the equator, may be directed so as to intersect the same lines of force in the contrary direction, and then return along a line of force to its commencement; and so two surface sections may be compared. It is manifest that every loop forming a complete circuit, which is in a great plane passing through the axis of the magnet, must have precisely the same lines of force passing into and passing out of it, though they may, so to say, be expanded in one part and compressed in another; or (speaking in the language of radiation) be more intense in one part and less intense in the other. It is also as manifest, that, if the loop be not in one plane, still, on making one complete revolution, either with or without the magnet, it will have intersected in its two opposite parts an exactly equal amount of lines of force.

Hence the comparison of any one section of a given amount of lines of force with any other section is rendered, experimentally, very extensive.

3111. Such results prove, that, under the circumstances, there is no loss, or destruction, or evanescence, or latent state of the magnetic power by distance.

3112. Also that convergence or divergence of the lines of force causes no difference in their amount.

3113. That obliquity of intersection causes no difference. It is easy so to shape the loop (3110.), that it shall intersect the lines of force directly across at both places of intersection, or directly at one and obliquely at the other, or obliquely in any degree at both; and yet the result is always the same (3093.).

3114. It is also evident, by the results of the rotation of the wire and magnet (3097. 3106.), that when a wire is moving amongst equal lines (or in a field of equal magnetic force), and with an uniform motion, then the current of electricity produced is proportionate to the *time*; and also to the *velocity* of motion.

3115. They also prove, generally, that the quantity of electricity thrown into a current is directly as the amount of curves intersected.

3116. In addition to these results, this method of investigation gives much insight into the internal condition of the magnet, and the manner in which the lines of force (which represent truly all that we are acquainted with of the peculiar action of the magnet) either terminate at its exterior, or at any assumed points, to be called poles; or are continued and disposed of within. For this purpose, let us consider the external loop (3093.) of fig. 5. When revolving with the magnet no current is produced, because the lines of force which are intersected on the one part, are again intersected in an opposing direction on the other (3110.). But if one part of the loop be taken down the axis of the magnet, and the wire then pass out at the equator (3091.), still the same absence of effect is produced; and yet it is evident that, external to the magnet, every part of the wire passes through lines of force, which conspire together to produce a current; for all the external lines of force are then intersected by that wire in one revolution (3101.). We must therefore look to the part of the wire *within* the magnet, for a power equal to that capable of being exerted externally, and we find it in that small portion which represents a radius at the central and equatorial parts. When, in fact, the axial part of the wire was rotated it produced no effect (3095.); when the axial, the inner radial, and the external parts were revolved together, they produced no effect; when the external wire alone was revolved, *directly*, it produced a current (3091.); and when the internal radius wire alone (being insulated from the magnet) revolved, *directly*, it also produced a current (3095. 3098.) in the contrary direction to the former; and the two were exactly equal in power; for when both portions of the wire moved together *directly*, they perfectly compensated each other (3095.). This radius wire may be replaced by the magnet itself (3096. 3118.).

3117. So, by this test there exist lines of force within the magnet, of the same *nature* as those without. What is more, they are exactly equal in *amount* to those without. They have a relation in *direction* to those without; and in fact are continuations of them, absolutely unchanged in their nature, so far as the experimental test can be applied to them. Every line of force therefore, at whatever distance it may be taken from the magnet, must be considered as a closed circuit, passing in some part of its course through the magnet, and having an equal amount of force in every part of its course.

3118. When the axial part of the wire is dismissed and the magnet employed in its place, so as to be included in the circuit, it is easy to see how it acts the part of the conductor. For suppose the wire itself to be continued from N to *b*, fig. 12, by any of the three paths indicated by dotted lines, the effect is the same in all the cases, both by experiment (3093.) and by principle (3100.). For whatever the form of the path, it will in one revolution intersect the same amount of lines of force within the magnet, as are intersected in the contrary direction by the part of the wire outside the magnet; and when the magnet is employed to complete the circuit in place of the internal wire, then its substance produces precisely the same result; for direction and every other circumstance which influences the result remains the same: one conductor has simply been substituted for another. The great mass of the magnet might be supposed able to do something more than the thin wire, but the reason why it only equals it in effect will be seen hereafter (3137.). And as the axial wire, in revolving, does nothing but conduct (3095.), all the effect being produced by that part which represents a radius between the axis and the equator (3098.); so the magnet, revolving as a cylinder, is as to its mass like the revolving wire; with the exception of so much of it as represents a radius connecting together the two points at the pole or axis and at the equator, where communication with the wire is completed. As was shown long ago (220.), if a cylinder magnet be revolved, and the ends of the galvanometer wires *a c* be applied to the extremities of its axis, no current is evolved; but if *a* be applied to one end, it matters not which, and *c* be applied at the equator or any other part on the surface of the cylinder, a current always in the same direction for the same rotation will be produced.

Fig. 12.

3119. Further to prove these points, the magnets were cut in half through the equatorial plane, and then, either a disc of copper placed there, or a wire radius only, or the magnets brought together again: and these three arrangements were used in succession to complete the circuit from the axial wire (3095.) to a fixed wire at the surface of the equator. Whichever was employed the current produced was the same, both in direction and amount. If the cylinder magnet above described (3118.) be terminated at the ends by attached discs of silver or copper, the wires applied to their surfaces, as they revolve with the magnet, produce precisely the same currents as to direction as if applied to the surface of the magnet itself (218. 219.).

3120. In this striking disposition of the forces of a magnet, as exhibited by the moving wire, it exactly resembles an electro-magnetic helix, both as to the direction of the lines of force in closed circuits, and in their equal sum within and without. No doubt, the magnet is the most heterogeneous in its nature, being composed, as we are well-aware, of parts which differ much in the degree of their magnetic development; so much so, that some of the internal portions appear frequently to act as keepers or submagnets to the parts which are further from the centre, and so, for the time, to form complete circuits, or something equivalent to them, within. But these make no part of the resultant of force externally, and it is only that resultant which is sensible to us in any way; either by the action on a needle, or other magnets, or soft iron, or the moving wire. So also the power which is manifest *within* the magnet by its effect on the moving mass, is still only that same resultant; being equal to, and by polarity and other qualities, identical with it. No doubt, there are cases, as upon the approach of a keeper to the poles, or the approximation of other magnets, either in favourable or adverse positions, when more external force is developed, or it may be a portion apparently thrown inwards and so the external force diminished. But in these cases, that which remains externally existent, corresponds precisely to that which is the resultant internally; for when either the same, or contrary poles, of a powerful horseshoe magnet were placed within an inch and a half of the poles of the bar magnets, prepared to rotate with the attached wires (3092.), as before described, still, upon their revolution, not the slightest action at the galvanometer was perceived; the forces within the magnet and those without perfectly compensating each other.

3121. The definite character of the forces of an invariable magnet, at whatever distance they are observed from the magnet, has been already insisted upon (3109.). How much more strikingly does that point come forth now, that, being able to observe within the magnet, we find the same definite character there; every section of the forces, whether within or without the magnet, being exactly of the same amount! The power of a magnet may therefore be easily represented by the effects of *any* section of its lines of force; and as the currents induced by two different magnets may easily be conducted through one wire, or be, in other ways, compared to each other, so facilities may thus arise for the establishment of a standard amongst magnets.

3122. On the other hand, the use of the idea of *lines of force*, which I recommend, to represent the true and real magnetic forces, makes it very desirable that we should find a unit of such force, if it can be attainable, by any experimental arrangement, just as one desires to have a unit for rays of light or heat. It does not seem to me improbable that further research will supply the means of establishing a standard of this kind. In the mean time, for the enlargement of the utility of the idea in relation to the magnetic force, and to indicate its conditions graphically, lines may be employed as representing these units in any given case. I have so employed them in former series of these Researches (2807. 2821. 2831. 2874. &c.), where the direction of the *line of force* is shown at once, and the relative amount of force, or of lines of

force in a given space, indicated by their concentration or separation, *i. e.* by their number in that space. Such a use of unit lines involves, I believe, no error either in the direction of the polarity or in the amount of force indicated at any given spot included in the diagrams.

3123. The currents produced in wires, when they cross lines of magnetic force, are so feeble in intensity (though abundant enough in quantity, as many results show), that a fine wire galvanometer must of necessity offer great obstruction to their passage. Therefore, before entering upon further experimental inquiries, I had another galvanometer constructed, in which the needles belonging to that made by RUHMKORFF were employed, but the coil was replaced by a single convolution of very stout wire. The wire was of copper, 0·2 of an inch in diameter. It passed horizontally under the lower needle, then, as nearly as might be, between that and the upper needle, over the upper, and then again between that and the lower needle, fig. 13, and was afterwards attached to the stand, and continued for 19 or 20 feet outside of the glass cover. Such a wire had abundant conducting power; and though it passed but once round each needle, gave a deflection many times greater than that belonging to the former galvanometer. Thus when the ends of the 19 feet of wire were soldered together, so as to form one loop or circuit, the passage of the wire once between the poles of a horseshoe magnet (3124.), caused a deflection, or rather swing of the needle of above 90°. I have had a more perfect instrument, of the same kind, constructed, in which the conducting coil was cut out of plates of copper, so as to form a square band 0·2 of an inch in thickness, which passed twice round the vibration plane of each needle, as represented, fig. 14. The length of metal around the needles was 24 inches, and the galvanometer was very sensitive, but the experiments to be described were made chiefly with the former instrument.

Fig. 13.

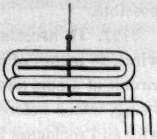
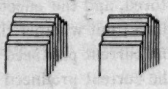


Fig. 14.

3124. It was necessary, first, to ascertain the effect of certain circumstances upon this simple galvanometer, as to their modification of its indications. The magnet to be used was a compound horseshoe instrument, weighing 16 lbs., and able to support 40 lbs. by the keeper or submagnet. It is some years since it was magnetized, and it is therefore, probably, in a nearly constant state as to power. The poles have the form delineated, fig. 15. Their distance apart is 1·375 inch, and the distance downwards, from their summit to the bottom or equator of the magnet, is 8·5 inches. The galvanometer stood in the prolongation of the magnetic axis, *i. e.* the line from pole to pole, and whether it were 6 or only 3 feet distant, was

Fig. 15.



hardly at all affected in the time of its vibration, being adjusted so nearly astatic as to require about ten seconds to swing to the right or to the left.

3125. On passing the wire across the magnetic field, as just described (3123.), but with different velocities, effects different in degree were obtained at the galvanometer, for the reasons formerly given (3104. 3106.). The quickest velocity gave the greatest result, equal at times to 140°, whilst a very slow motion gave only 30° or 40°. Still with moderately quick velocities the effects were nearly alike, and by operating with the same velocity, and taking the average of several observations, a very uniform result could be obtained.

3126. On cutting the wire across, and then putting the ends together in various ways, it was found that great care was requisite in making contact, in this or in similar cases. Thus, to press the ends lightly together was not sufficient; they required to be well and recently cleaned and pressed closely into contact. Junctions effected by soldering or dipping into cups of mercury were still better, when made with care, and were employed at the galvanometer and elsewhere as often as possible.

3127. To ascertain generally the obstruction caused by the interposition of thin wires, 28 inches of copper wire, 0·045 of an inch in diameter, were introduced into the circuit at a part away from the magnet, with excellent junctions. The oscillation or swing, which before was 140° or more, was now reduced to 40°. On taking out the wire and replacing it by another, also of copper, but only 19·5 inches in length, and 0·0135 in diameter, the deflection was reduced to 7° or 8°.

3128. For a rough comparison of the power of this magnet and the former bar magnets (3085.), by the present galvanometer, the thick wire was bent into a loop (3086.), and the two bar magnets, with like ends together, passed quickly through it up to the equatorial part; the deflection was about 30°. Such a passage intersected nearly all the lines of force of the bar magnets. A similar motion of the magnets close to, but outside of, the loop, produced no effect at the galvanometer.

3129. In respect of the alteration of the lines of force, either in position or in total amount, by bringing the poles of the horseshoe magnet (3124.) much nearer together, the following experiments were made. The distance between the poles is 1·375 inch; by placing a cube of soft iron, 0·8 of an inch in the side, within this space, it was diminished to 0·575, and thus, virtually, the distance apart much lessened, and, as was afterwards shown experimentally (3130.), the external power of the magnet concentrated there. Then, whilst the cube was in place, the thick wire of 0·2 of an inch in diameter, was arranged so as to pass across the magnetic axis or place of strongest action, and fixed; after which the iron cube was alternately removed and again restored, and the effects observed. Feeble electric currents were produced at these times; but whether the cube was put into its place from below, or above, or the sides, the current produced was always in the same direction; and when it was removed the current produced was in the reverse direction. If the cube were carried up to,

by, and away from, the magnetic axis in one motion, then there was no effect at the galvanometer. On the other hand, when the wire was carried across the magnetic field as described (3123.), so as to intersect all the lines of force in one movement, and sum up their power at the galvanometer, then there was no difference in the result, whether the iron cube was in its place or not; showing, as far as this apparatus could indicate, that the sum of power in the section of all the lines of force external to the magnet, was the same under both circumstances, though the distribution of it was different.

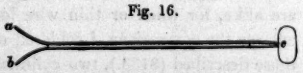
3130. The very action produced by the cube, when in and out of place (3129.), upon the forces which affected the stationary wire, was a proof of the difference of distribution at different times.

3131. A block of bismuth, employed in place of the iron cube, had no sensible effect upon the wire whether it were still or moving.

3132. This galvanometer was first employed for a repetition of all the former experiments with the bar magnets (3091. &c.). The results were absolutely the same, except that the amount of the deviation produced, when deviation was a result, was larger than in the former cases.

3133. For the comparison of different thicknesses of the same metal, I took copper wires in lengths of 10·5 inches, and different diameters, and bending them into loops of a form and size such as would admit them to pass with facility over a pole of the horseshoe magnet, soldered them to the ends of two conducting rods, made of copper wire 0·2 of an inch in diameter and 35 inches in length each, which were fixed on opposite sides of a narrow slip of wood. The whole arrangement is seen in fig. 16; the terminations *a b* dip into the mercurial cups of the galvanometer, the parts at *c* are brought so close together as to touch, except for the intervention of a piece of card, and thus the parts from *c* to *a b* are thrown out of action, except as mere conductors, whilst the loop, being made to descend over one magnetic pole, intersects very nearly the whole of the magnetic curves, and always the same proportion.

Fig. 16.



3134. The former magnet was too powerful for comparative experiments, therefore a smaller one was employed, consisting of five plates, weighing 8 lbs., and able to carry 21 lbs. easily at the keeper. The poles were 1·2 inch apart and an inch thick each, in the direction of the magnetic axis. If less magnetic power were required, an adjustment was easily made, by applying the keeper to the side upon both limbs, the magnetic communication being effected either nearer to the poles, or nearer to the equator or bend, as less or more power was required. The descent of the loop between the poles is then best regulated by causing the conductor wires to bear ultimately against a stopping-block.

3135. The effect of a quick and a slow motion was found to be the same as before (3104. 3105.). Such velocities as the hand could impart were very effectual, and gave results of very considerable uniformity when quick motions were employed.

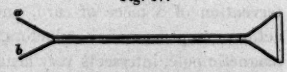
3136. Three different loops were compared together, consisting of copper wire, the diameters of which were 0·2, 0·1 and 0·05 of an inch, or as 4, 2 and 1; their sectional areas or masses therefore were as 16, 4 and 1. Ten or twelve observations were made with each loop; the results were near together, and the average for each loop, being the extent of the swing declination on one side from zero, is as follows:—

Copper wire of $\frac{1}{20}$th of an inch in thickness	16°·00
Copper wire of $\frac{1}{10}$th of an inch in thickness	44°·40
Copper wire of $\frac{1}{5}$th of an inch in thickness	57°·37

Now though the thicker wires produced the largest effect, the results were evidently not at all in proportion to the masses of the wires; the smaller having greatly the advantage in that respect. On the other hand, when four of the smaller wires were placed side by side, so as to form one loop equal in mass to the second loop, it gave the same result as that loop, being of the same power.

3137. The disproportion of the *difference* of these three wires is evidently a consequence of the relative difference of the mere conducting part of the circuit. To compare accurately the effect of the lines of force on wires of different diameters moving across them, these diameters should continue to, and through the galvanometer (205.), otherwise the thin wire current has an advantage given to it in the conducting part, which the thick wire current has not. Hence the reason why a thin wire galvanometer, such as that before described (3086.), gives results which are alike, for thick or thin wire loops, or for fasciculi of few or many wires. To enlarge the comparison, I soldered on to two pairs of conductors, the dimensions of those described (3133.), two cylinders of copper, each 5·5 inches long, but one was only 0·2 of an inch thick and the other 0·7, or twelve times the mass of the first, fig. 17. They were then passed in succession between the poles of the magnet, and gave results very nearly alike.

Fig. 17.



If there was any difference, the effect was highest with the smallest cylinder; and this may very well be; for as the magnetic field was not equal in force, but most intense in the magnetic axis, so it is evident, that whilst one part of the large cylinder, in passing across, was at the axis, other parts were in places of less intense force and action, and so a return current may have existed in them, which could not occur to the same extent in a cylinder little more than a fourth of the diameter of the former, and which, at the same time, had an outlet for the currents equal to its own diameter, through the conducting wires. A similar relation of mass occurs in the case where the body of the magnet itself, in revolving, does no more than a small radial wire within it (3118.).

3138. The influence of this lateral conduction (3137.), in cases of magneto-electric conduction, must be well understood; otherwise, in the application of the principles to investigation, errors will frequently creep in. Their effect may be shown in the following instances:—a loop of four wires, 0·048 of an inch in diameter (3136.), was passed over the pole of the magnet, and produced a certain result of deflection or swing; when the wires were separated two and two, so as to be half or three-quarters of an inch apart, and when, therefore, in moving across the magnetic field, one pair went before the others, the effect was less, for the reason already given in the case of the copper cylinder (3137.). When three wires were allowed to go by together, but one taken aside a couple of inches, the effect fell very much; and when that fourth one was cut across to prevent the return current in it, the effect of the three rose at the galvanometer very greatly, almost equalling the effect of the four when together.

3139. A loop was constructed of seventy-six equal fine copper wires, each 10·5 inches long and 0·0125 of an inch in diameter, and its effect observed when more and more of the wires were cut away. As it is the comparison of the smaller numbers of wires, one with the other, that is of most value, I will give the averages of each number for several observations, in the reverse order in which they were obtained; and I introduce the results with larger numbers of wires only for the general purpose of showing how the effect passes into that with the cylinder of copper (3137.), the galvanometer conductors always being of the same length and thickness.

1 wire produced an average swing of	8·3
2 wires produced an average swing of	15·3
3 wires produced an average swing of	21·8
4 wires produced an average swing of	27·9
5 wires produced an average swing of	34·4
6 wires produced an average swing of	37·8
8 wires produced an average swing of	50·1
12 wires produced an average swing of	65·1
16 wires produced an average swing of	80·5
26 wires produced an average swing of	118·0
36 almost swung the needle round.	
46 stronger than the last.	
56 swung the needle quite round.	
66 a little stronger.	
76 stronger: swung the needle freely round the circle.	

Each time that the needle passed 180°, it was returned, that the torsion force might remain the same for every case.

3140. When the loop of four equal wires (3136.) was employed, so arranged that, in respect of the part which passed between the poles, they should be close together in one plane, it made no difference in the result, whether that plane was perpendicular

to the magnetic axis or parallel to it; *i. e.* whether the wires in moving, formed a band which moved edgeways or flat ways; the results were the same as with the four wires close together, so as to represent, as far as they could, a round or square wire.

3141. From all these results it may be concluded, that the current or amount of electricity evolved in the wire moving amongst the lines of force, is not, simply, as the space occupied by its breadth correspondent to the direction of the line of force, which has relation to the *polarity* of the power, nor by that width or dimension of it which includes the number or *amount* of the lines of force, and which, corresponding to the direction of the motion, has relation to the *equatorial* condition of the lines; but is jointly as the compound ratio of the two, or as the mass of the moving wire. The power acts just as well on the interior portions of the wire as on the exterior or superficial portions, and a central particle, surrounded on all sides by copper, is just in the same relation to the force as those which, being superficial, have air next them on one side.

3142. By immersing the poles of the magnet in different media, and then making comparative experiments with the same copper wire loop (3145.), it was found that the amount of the induced current was the same in air, water, alcohol and oil of turpentine. The experiments in air were repeated between those with the liquids, so as to give a very consistent and safe result as to the equality of action in all the cases.

3143. The effect of *variation of substance* was the next subject which seemed to me important to bring under investigation, because it has a direct relation to the amount of force exerted, or ready to be exerted, within solid bodies, at any distance from the magnet, in situations and under circumstances where it was absolutely impossible to apply the vibrations of a magnetic needle, or any other form of the effects of attractive and repulsive forces. The interior of such bodies as iron, copper, bismuth, mercury, &c., including the most paramagnetic and the most diamagnetic, seemed, in this way, open to experimental investigation, both as to the amount of lines of force traversing them under various circumstances, and also as to the direction of the lines or their polarity.

3144. In an early series of these Researches*, experiments bearing upon this subject are described (205–213.). Wires of different metals were moved across the lines of force of a magnet, and the result arrived at was, that the currents induced in these different bodies were proportional to their electro-conducting power (202. 213.).

3145. The thick wire galvanometer (3123.), with its good and short conducting communications, promised however better results, and therefore loops like those already described of copper wire (3133.), were prepared with wires of different metals, all of the same diameter, namely, 0·04 of an inch, being only $\frac{1}{15}$th of the substance of the conducting and galvanometer wire. The metals were copper, silver, iron, tin,

* Philosophical Transactions, 1832, pp. 179–182.

lead, platinum, zinc. Under these circumstances the substance concerned in the excitement of the current is made to vary, whilst the conducting part of the system is very good and remains the same. The results with these loops were as follows, being the average of from six to ten experiments for each loop:—

Copper	63·0
Silver	61·9
Zinc	31·5
Tin	19·1
Iron	18·0
Platinum	16·9
Lead	12·1

3146. In order to dismiss, as much as possible, the obstruction caused by bad conducting power, and bring out any difference that might exist between paramagnetic and diamagnetic metals, three metals were selected, namely, tin, iron and lead in wires, as before, of 0·04 of an inch diameter; but the length was restricted to 3 inches, instead of extending to 10·5 inches, and the rest of the loop was made up of the conducting copper wire of 0·2 in diameter, as in fig. 18.

Fig. 18.

Of course, the effect of the whole loop is a mixed effect, being partly due to the power represented by the lines intersected by the thick copper portion, and partly by those intersected by the three inches of special wire passing between the poles. But as the great amount of force is concentrated within a space not more than an inch and a half or 2 inches in extent (as is seen on carrying any of the loops across the magnetic axis), and as even that could be made still more concentrated by using the iron cube (3129.), and so bringing the poles virtually nearer to each other, it was hoped that the chief effect would be there, and so any peculiar difference existing between iron on the one hand and tin and lead on the other, be rendered manifest, especially as the resistance to conduction was greatly diminished by shortening the wires from 10·5 to 3 inches.

3147. The many experiments made with each metal were very close together. The average of the results for the three metals was as follows:—

Tin	37·1
Iron	34·8
Lead	25·4

The proportions, and therefore the results, are almost identical with those obtained before (3145.).

3148. When lead and copper, arranged at the bar magnets (3084. 3085.), had been compared in former experiments with each other by the fine wire galvanometer, the results for both had been the *same*. But then the two wires used were short, and

far thicker than the wires of the galvanometer or of the conducting circuit, and were therefore limited in the production of their peculiar action, by those circumstances of mass already described (3137.). To show that that was the case, I now, with the thick wire galvanometer, employed two equal loops of copper and iron wire, 0·2 of an inch in thickness, fig. 16 (3133.), passing them equably over the pole of the small horseshoe magnet, reduced by the keeper (3134.). The results were very consistent, and the mean of them was, for

Copper	41°·7
Iron	33·7

3149. Here, therefore, the difference between copper and iron is not so great as that of 1 to 1·24; whilst when the conductors, not concerned in the excitement, were very good, and able, comparatively, to carry on to the galvanometer nearly all the effect of the excitement, it was as great as 1 to 3·5, the difference being in the latter case above tenfold what it is in the former.

3150. To raise the effect dependent upon the mass in relation to that of the conducting wires to a still higher degree, I had a cylinder of iron, 5·5 inches in length and 0·7 of an inch in diameter, soldered on to the ends of conducting wires, so as to be in all respect like that of copper before described (3137.). In this case the iron not only rose up to the copper in effect, but even surpassed it; the results being for copper 35°·66, and for iron 38°·32. Thus, under these circumstances of mass, the difference between iron and copper disappears. The apparent inferiority of copper is probably due to the lateral discharge, which before reduced the effect of a cylinder below that of a thick wire (3137.). The iron being a worse conductor in itself, and having equally good conductors in the prolongation of the circuit as when it was employed as wire, would, I think, have proportionately less lateral discharge in it than the copper.

3151. For a comparison, both as regards the particular substance and the mass, I attached a similar cylinder of bismuth to conductors. Its effect, with the same magnet and force, was 23°; a very high proportion in relation to the copper, and no doubt due to its mass. If it could have been compared as a wire, only 0·04 in diameter (3145.), it would probably have appeared almost indifferent (3127.)*.

3152. So the current of electricity excited in different substances, moving across lines of magnetic force, appears to be directly as the conducting power of the substance. It appears to have no particular reference to the magnetic character of the body, for iron comes between tin and platinum, presenting no other distinction than

* When bismuth is soldered into the circuit, it requires to be left a long time before it is used for experiments, and should then be covered up, and the loop handled with great care; otherwise thermo-currents are produced. For an hour or two after soldering it generates electrical currents, which appear at the galvanometer very irregularly, being probably due to internal molecular changes, which occur from time to time until the whole has acquired a permanent state of equilibrium.

that due to conducting power, and differing far less from them, than they do from other metals not magnetic.

3153. The amount of *lines of force* (and of the force represented by them) appears, therefore, to be equal for equal spaces occupied by tin, iron, and platina under the circumstances; for the difference in result is in no proportion to the ordinary magnetic difference, and only as the conducting power. This agrees with the conclusion before arrived at, that, for air, water, bismuth, oxygen, nitrogen, or a vacuum, the lines of force are the same in amount, except as they are more or less concentrated in the substance across which they pass (2807.), according as it is more or less competent to conduct (2797.) or transmit the magnetic force.

3154. Such a conclusion as that just arrived at, brings on the question of what is *magnetic polarity*, and how is it to be defined? For my own part, I should understand the term to mean, the opposite and antithetical actions which are manifested at the opposite ends, or the opposite sides, of a limited (or unlimited) portion of a line of force (2835.). The line of dip of the earth, or a part of it, may again be referred to as the natural case; and a free needle above or below the part, or a wire moving across it (3076. 3079.), will give the direction of the polarity. If we refer to an entirely different and artificial standard as the electro-magnetic helix, the same meaning and description will apply.

3155. If the term *polarity* have any meaning, which has reference to experimental facts and not to hypotheses only, beyond that included in the above description, I am not aware that it has ever been distinctly and clearly expressed. It may be so, for I dare not venture to say that I recollect all I have read, or even all the conclusions I myself have at different times come to. But if it neither have, nor should have, any other meaning, then the question arises, is it correctly exhibited or indicated in every case by attractions and repulsions, *i. e.* by such like mutual action of particular bodies on each other under the magnetic influence? A weak solution of protosulphate of iron, if surrounded by water, will, in the magnetic field, point axially; if in a stronger solution than itself, it will point equatorially (2357. 2366. 2422.). The same is true with stronger cases. We cannot doubt it will be true even up to iron, nickel, and cobalt, if we could render these bodies fluid in turn without altering their paramagnetic power, or if we had the command of magnets and of paramagnetic and diamagnetic media, stronger or weaker at pleasure. But in the case of the solutions, we cannot suppose that the weaker has one polarity in the stronger solution and another in the water. The lines of force across the magnetic field have the same general polarity in all the cases, and would be shown experimentally to have it, by the moving wire (3076.), though not by the attractions and repulsions.

3156. Here, therefore, we have a *difference* in the two modes of experimental indication; not merely as to the method, but as to the nature of the results, and the very

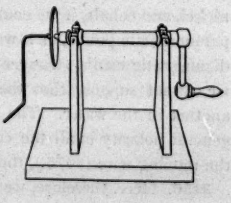
principles which are concerned in their production. Hence the value I think of the moving wire as an investigator; for it leads us into inquiries which touch upon the very nature of the magnetic force. There is no doubt that the needle gives true experimental indications; but it is not so sure that we always interpret them correctly. To assume that pointing is always the direct effect of attractive and repulsive forces acting in couples (as in the cases in question, or as in bismuth crystals), is to shut out ideas, in relation to magnetism, which are already applied in the theories of the nature of light and electricity; and the shutting out of such ideas *may be* an obstruction to the advancement of truth and a defence of wrong assumptions and error.

3157. What is the idea of polarity in a field of *equal force*? (whether it be occupied by air or by a mass of soft iron?) A magnetic needle, or an oblong piece of iron, would not show it in the air or elsewhere, except by disturbing the equal arrangement of the force and rendering it unequal; for on that the pointing of the needle or the iron, or the motions of either towards the walls of the magnetic field, if limited (2828.), would depend. A crystal of bismuth in showing this polarity by position (2464. 2839.), does it without much altering the distribution of the force, and the alteration which does take place is in the contrary direction to that effected by iron (2807.), for it expands the lines of force. It seems readily possible that a magne-crystal might exist, which, when in its stable position, should neither cause the convergence nor divergence of the lines of force within it. It need only be neutral in relation to space or any surrounding medium in that direction, and diamagnetic in its relation in the transverse direction, and the conditions would be fulfilled.

3158. But though an ordinary magnetic needle* cannot show polarity in a field of equal force, having no reference to it, and in fact ignoring such a condition of things, a moving wire makes it manifest instantly, and also shows the full amount of magnetic power to which such polarity belongs; and this it does without disturbing the distribution of the power, as far as we comprehend or understand distribution, when thinking of magnetic needles. At least such at present appears to me to be the case, from the consideration of the action of thin and thick wires (3141.) and wires of different substances (3153.).

3159. As an experimentalist, I feel bound to let experiment guide me into any train of thought which it may justify; being satisfied that experiment, like analysis, must lead to strict truth if rightly interpreted; and believing also, that it is in its nature far more suggestive of new trains of thought and new conditions of natural power. In order to extend its indications, and vary the form in which the principle of the moving wire may be applied, I had an apparatus constructed, fig. 19, consisting of a wooden axis, one extremity of which was terminated by a copper screw,

Fig. 19.



* One could easily imagine hypothetically a needle that should do so.

intended to receive and carry one or more discs of metal that might be screwed on to it. This end projected so far beyond the support, that such discs could be partly introduced between the poles of a horseshoe magnet, so as when revolving, to move across the lines of force at their most intense place of action; and, whilst the magnet and the apparatus continued fixed, to revolve continuously across the same lines of force. One of the galvanometer wires was pointed, and so held as to bear into and against the surface of a cup-shaped cavity at the end of the axial screw; and the other was applied by the hand, or so fixed as to bear by a rounded part against the rim of the disc, at that point which was furthest within the poles of the magnet.

3160. Discs of metal were prepared for this apparatus, each 2·5 inches in diameter, and of different thicknesses and material. When a disc of copper was fixed on the axis, and adjusted in association with the large horseshoe magnet (3159.), as described above, three, or even two revolutions of it, would deflect the needle of the thick wire galvanometer through a swing of 30°. In this apparatus, the most effectual part of the portion of the disc which is at any moment passing across the magnetic axis, is that which is near the circumference; for it has the greatest velocity, consequently moves through more space, and that in a part where the lines of force are most concentrated.

3161. The contact at the end of the axle should always be carefully watched and made good. The degree of pressure on the edge of the disc should not be too slight; otherwise the contact, under the circumstances of the motion, is not sufficient to carry forward the same constant proportion of current generated. Neither should it be made at the angles of the disc edge; if a grating or cutting friction occur, an electric current is generated by it. With a smooth hard friction of copper wire against the copper disc there is very little evolution of current. When the copper wire presses against the edge of an iron disc there is far more. In either case, however, the effect may be eliminated or compensated; for, in whichever direction the disc is revolved *without* the magnet, the deviation of the needle, if any be produced, remains the same; whereas, when the magnet is in place, the deviations produced by it are in the reversed direction for reversed revolutions. Hence, if an equal number of revolutions be made in the two directions, and the unequal deflections in opposite directions be noted, the half of their sum will give nearly the amount of deflection which would have occurred if no current had been exerted by friction at the edge, *i. e.* provided the deflections have not been through large arcs. These effects of friction are no doubt objections to the principle in this form; still the results are, as it appears to me, valuable in relation to copper and iron, and are as follows.

3162. A copper disc, 0·05 of an inch in thickness, gave a swing deflection for two revolutions, which, being the average of several experiments, $=20°{\cdot}8$. A second copper disc, of 0·1 of an inch in thickness, gave an average deflection of $27°{\cdot}8$. A third copper disc, of 0·2 in thickness, gave a deflection of $26°{\cdot}5$. Here, therefore,

not only has the thickness (with these conditions of contact) been attained for the maximum effect, but even surpassed (3137.). Then an *iron* disc of 0·05 in thickness, was placed on the axle, and gave, as its mean result, a deflection of 15°·4. Another iron disc of four times the thickness (or 0·2) gave a deflection only of 14°. So here also, as before, the thickness of maximum effect had been surpassed.

3163. The two discs of copper and iron of 0·2 in thickness each, which had produced separately the respective deviations of 26°·5 and 14°, were then both fixed on the axle, being separated from mutual contact in respect of their mass, by a disc of paper, though both were of course in contact at the centre of motion with the copper axle, by means of which the electric communication was perfected. In arranging their place between the poles of the magnet, the iron was placed at mid-distance, and therefore the copper a little on one side. When the copper disc was brought into the circuit, it, by two revolutions, gave an average deviation of 23°·4; and when the iron disc was in the circuit, the deviation produced by it was 11°·91. Here, therefore, the proportions were nearly the same, when the two discs were subject at the same moment to the magnetic power, as when they were examined separately. Both have fallen a little, but not in any manner which seems to indicate that the iron has had any peculiar influence in altering or affecting the lines of force passing across the magnetic field. The effect which has taken place, appears to be one due to the action of the collateral mass of conducting matter.

3164. If the direction of the electric current induced by the magnetic force in the moving metal be taken as the true indication of polarity, and, I think, it cannot be denied that it represents that character of the force, which the term polarity is intended to express, and is unchangeably associated with that character; then these results show that the polarity of the lines of force within the iron is the same with that within the copper, when both are submitted in like manner to the magnetic force. In association with the former and new results with *bismuth* (2431. 3151. 3168.), and numerous other phenomena, the same conclusion may be drawn as to the lines of force within that substance, for the effects are the same with regard to the production of a current in it; and so further evidence is added to that which I have given, tending to show that bismuth is not polarized in the reverse direction as iron or a magnet (2429. 2640.). By reference to the phenomena presented by the relative actions of paramagnetic and diamagnetic substances, the same conclusions may be drawn with respect to all bodies and to space itself (2787. &c.).

3165. That the iron disc affects the disposition of the *lines of force*, is no doubt true, and the extent to which this is done is easily seen, by fixing a small magnetic needle, about 0·1 or 0·05 of an inch in length, across the middle of a piece of stretched thread as an axis, and then bringing it into the magnetic field and near the edges of the stationary disc. The lines of force will be seen (3071. 3076.) gathering in upon the iron at and near its edge, but only for a very little distance from it in any direction: the effect is that which I have considered proper to a para-

magnetic body (2807.). Elsewhere, the lines of force go with the same direction across the magnetic field where the iron is, as where it is not; and it is to me a proved fact, proved by the numerous results given, that a section of the lines of force taken across the magnetic field through the air, where there is no iron, is exactly equal in amount of force to a section taken across parallel to and through the iron disc (3163.). All iron under induction must have just as much force, *i. e.* lines of force in its internal parts, as is equivalent to the lines which fall on to, and are continued through and out of it; and the same is true, as it appears to me, of any other paramagnetic or diamagnetic substance whatever. The same is true *for the magnet itself*; for a section through the magnet has been shown to be exactly equal to a section anywhere through the outer lines of force (3121.), and these sections may be taken at the surface of the magnet, where they may be considered as either in the air or in the magnet indifferently; and therefore alike in size, shape, power, polarity, and every other point.

3166. I have used the phrase *conduction polarity* on a former occasion (2818. 2835.), but so limited, that it could lead to no mistake of my meaning, either then or now. It requires no words to show how it is included in the higher and general expression of the direction or polarity of the lines of force.

3167. Some other results with the disc apparatus (3159.) were obtained, which it may be useful to describe here. *Tin* was formed into a disc of 0·1 in thickness, and 2·5 inches in diameter. The effect of the friction of the copper conductor at its edge, was a feeble current, the reverse of that produced in the cases of copper and iron (3161.); but the current produced by the revolution, and dependent on the polarity of the lines of force, was the same as before. It produced a swing deflection of 14°·9 for two revolutions of the disc.

3168. A disc of *bismuth* produced far too strong a current by friction against the copper conductor, to allow of any useful result in its simple state. A ring of copper foil was therefore formed, and being placed tightly on the bismuth disc, was wedged up by plates of clean copper foil, so as to produce a clean hard contact; imperfect, no doubt, but as general as could be made under the circumstances. When this disc was rotated in the one direction, it gave a deflection in the same direction as if a copper or iron disc had been used; when rotated the other way, the deflection was little or nothing. This difference is due to the united influence of the rotation effect and the friction effect in the one case, and their opposition in the other; but the results show that the lines of force are in the same direction through bismuth, when between the magnetic poles, as they are through copper and iron. The induced current is small, both because of the bad conducting power of the bismuth and the imperfect contact at the edge. When the same copper rim was placed on the copper disc, it reduced the deflection of the needle from 26°·5 to 9°·34.

3169. In illustration of the effect produced by those parts of the disc, which, not being in the place of greatest action, are conducting back those currents formed by the radial parts in the place of maximum effect, I had a wooden disc constructed, 0·2 in thickness and 2·5 inches in diameter, the centre of which was copper, for the purpose of attachment and electrical connection, and the outer edge a ring of copper not more than $\frac{1}{10}$th of an inch in thickness. The two were connected by a single copper wire radius, in thickness 0·056 of an inch, which, as the disc revolved, was of course carried across and through the magnetic field. It gave a deflection of 14°. The copper disc of 0·05 thickness gave only an average of 28°. Now, though the matter of the copper ring round the wood will cause part of the current, yet the chief portion must be due to the copper radius, which, at the effectual part near the edge (3160.), is not more than the $\frac{1}{140}$th part of the full copper disc; and this indicates how much of the electricity put in motion there by the magnetic force must be returned back in short circuits in the other parts of the disc.

3170. The disc apparatus shows well the dependence of the induced current upon the *intersection* of the lines of force (3082. 3113.). If the disc be so arranged as to stand edgeways to the magnetic poles, and in the plane of the magnetic axis, so that it shall be *parallel* to the lines of force which pass by and through it, then no revolution of it, with the most powerful magnet, produces the slightest signs of a current at the galvanometer.

3171. The relation of the induced current to the electro-conducting power of the substance, amongst the metals (3152.), leads to the presumption that with other bodies, as water, wax, glass, &c., it is absent, only in consequence of the great deficiency of conducting power. I thought that processes analogous to those employed with the metals, might in such non-conductors as shell-lac, sulphur, &c., yield some results of static electricity (181. 192.); and have made many experiments with this view in the intense magnetic field, but without any distinct result.

3172. All the results described are those obtained with *moving metals*. But mere motion would not generate a relation, which had not a foundation in the existence of some previous state; and therefore the *quiescent* metals must be in some relation to the active centre of force, and that not necessarily dependent on their paramagnetic or diamagnetic condition, because a metal at zero, in that respect, would have an electric current generated in it as well as the others. The relation is not as the attractions or repulsions of the metals, and therefore not magnetic in the common sense of the word; but according to some other function of the power. Iron, copper, and bismuth are very different in the former sense, but when moving across the lines of force, give the same general result, modified only by electro-conducting power.

3173. If such a condition be hereafter verified by experiment, and the idea of an electrotonic state (60. 242. 1114. 1661. 1729.) be revived and established, then, such

bodies as water, oil, resin, &c., will probably be included in the same state; for the non-conducting condition, which prevents the formation of a current in them, does not militate against the existence of that condition which is prior to the effect of motion. A piece of copper, which cannot have the current, because it is not in a circuit (3087.), and a piece of lac, which cannot, because it is a non-conductor of electricity, may have peculiar but analogous states when moving across a field of magnetic power.

3174. On bringing this paper to a close, I cannot refrain from again expressing my conviction of the truthfulness of the representation, which the idea of lines of force affords in regard to magnetic action. All the points which are experimentally established with regard to that action, *i. e.* all that is not hypothetical, appear to be well and truly represented by it. Whatever idea we employ to represent the power, ought ultimately to include electric forces, for the two are so related that one expression ought to serve for both. In this respect, the idea of lines of force appears to me to have advantages over the method of representing magnetic forces by centres of action. In a straight wire, for instance, carrying an electric current, it is apparently impossible to represent the magnetic forces by centres of action, whereas the lines of force simply and truly represent them. The study of these lines has, at different times, been greatly influential in leading me to various results, which I think prove their utility as well as fertility. Thus, the law of magneto-electric induction (114.); the earth's inductive action (149. 161. 171.); the relation of magnetism and light (2146. and note); diamagnetic action and its law (2243.), and magnecrystallic action (2454.), are cases of this kind: and a similar influence of them, over my mind, will be seen in the further instances of the polarity of diamagnetic bodies (2640.); the relation of magnetic curves and the evolved electric currents (243.); the explication of Arago's phenomenon (81.), and the distinction between that and ordinary magnetism (243. 245.); the relation of electric and magnetic forces (1709.); the views regarding magnetic conduction (2797.), and atmospheric magnetism (2847.) I have been so accustomed, indeed, to employ them, and especially in my last Researches, that I may, unwittingly, have become prejudiced in their favour, and ceased to be a clear-sighted judge. Still, I have always endeavoured to make experiment the test and controler of theory and opinion; but neither by that nor by close cross examination in principle, have I been made aware of any error involved in their use.

3175. Whilst writing this paper, I perceive, that, in the late Series of these Researches, Nos. XXV. XXVI. XXVII., I have sometimes used the term *lines of force* so vaguely, as to leave the reader doubtful whether I intended it as a merely representative idea of the forces, or as the description of the path along which the power was continuously exerted. What I have said in the beginning of this paper (3075.) will render that matter clear. I have as yet found no reason to wish any part of those papers altered, except these doubtful expressions: but that will be rectified if

it be understood, that, wherever the expression *line of force* is taken simply to represent the disposition of the forces, it shall have the fullness of that meaning; but that wherever it may seem to represent the idea of the *physical mode* of transmission of the force, it expresses in that respect the opinion to which I incline at present. The opinion may be erroneous, and yet *all* that relates or refers to the disposition of the force will remain the same.

3176. The value of the moving wire or conductor, as an examiner of the magnetic forces, appears to me very great, because it touches the physics of the subject in a manner altogether different to the magnetic needle. It not only gives its indications upon a different principle and in a different manner, but in the mutual action of it and the source of power, it affects the power differently. The wire when quiescent does not sensibly disturb the arrangement of the force in the magnetic field; the needle when present does. When the wire is moving it does not sensibly disturb the forces external to it, unless perhaps in large masses, as in the discs (3163.), or when time is concerned (1730.), *i.e.* it does not disturb the disposition of the whole force, or the arrangement of the lines of force; a field of equal magnetic power is still equal to anything but the moving wire, whilst the wire moves across or through it. The moving wire also indicates quantity of force, independent of tension (2870.); it shows that the quantity within a magnet and that outside is the same, though the tension be very different. In addition to these advantageous points, the principle is available within magnets, and paramagnetic and diamagnetic bodies, so as to have an application beyond that of the needle, and thus give experimental evidence, of a nature not otherwise attainable.

Royal Institution,
October 9, 1851.

IV. *An Account of two cases, in which Ovules, or their Remains, were discovered in the Fallopian Tubes of Unimpregnated Women who had died during the period of Menstruation. By* H. Letheby, *M.B., Lond., Lecturer on Chemistry and Medical Jurisprudence in the Medical School of the London Hospital. Communicated by* T. B. Curling, *Esq., F.R.S.*

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THE observations that have been made at various times, during the last thirty years, by Messrs. Power, Lee, Barry, Wharton Jones, Girdwood, and others in this country, together with the experimental researches of MM. Valentin, Gendrin, Wagner, Bischoff, Pouchet and Raciborski on the Continent, have, I think, clearly proved that the phenomena manifested during the period of the catamenia in women, are closely connected with those observed during the time of heat or rut in quadrupeds; and that both of these phenomena are dependent on one cause, namely, the maturation of ovules. But while this hypothesis has been very generally admitted, there is, I believe, a tendency in the minds of many physiologists of the present day, to doubt whether the ovules so matured are ever extruded from the ovary and carried into the Fallopian tubes, without the stimulus of impregnation, or, at any rate, without the congress of the male. In support of this view, or rather of these doubts, an appeal is often made to the fact, that an ovule has never yet been detected in either of the Fallopian tubes of a virgin, who has died during the period of the catamenia, notwithstanding that many subjects have been examined, that most careful search has been instituted, and that appearances have frequently been noticed indicating the recent rupture of a Graafian follicle. In point of fact, it is imagined by those who entertain such doubts, that the fecundation of the germ takes place while it is within the Graafian follicle, and consequently, that if the ovule fails to be the subject of impregnation it never quits the ovary, but perishes within its formative vesicle. On the other hand, the researches of Bischoff have led him to enunciate a law, the purport of which is the very reverse of the preceding; for he says, that "the ovules formed in the ovaries of females of the human species and of mammiferous animals, undergo a periodical maturation, quite independently of the male seminal fluid. At these periods, known as those of heat or the rut in animals, and menstruation in the human female, the ovules which have become mature, disengage themselves from the ovary and are extruded. If the union of the sexes takes place, the ovule is fecundated by the direct action of the semen upon it. If no union of the

sexes occurs, the ovule is nevertheless extruded from the ovary, and enters the Fallopian tube, but there perishes*."

The law, as thus expressed, is in conformity with the opinions entertained by Drs. Robert Lee, Paterson, Girdwood, Gendrin, Pouchet, Raciborski, Mr. Wharton Jones, and many other authorities of the present time. It is also in accordance with the more ancient doctrines of Malpighi, Sir Everard Home, and Dr. Power. Nevertheless, as the truth of this law, in its application to the human female, appears to be still open to the evidence of positive proof, I have thought it desirable to publish a report of the two following cases.

Case 1st.—November the 20th, 1850, a woman aged twenty-six, in a state of great mental excitement, attempted self-destruction by cutting her throat. The wound which she inflicted was not dangerous; and after having been attended to by a surgeon for a few days, the woman was removed to the London Hospital, where she became a patient under Mr. Curling. She lingered until the 14th day of December, when she died. On the following day the body was examined, and it was noticed that the pelvic viscera were highly congested, that the uterus was considerably enlarged, that the vagina contained a sero-sanguineous fluid, and that the hymen was unruptured. In consequence of these appearances the parts were removed for further examination.

On cutting into the uterus, I discovered that it contained a small quantity of sanious fluid; I noticed, moreover, that both of the ovaries presented a number of stellate fissures or cicatrices on their surfaces; and that at one part of the left organ, namely, at its inferior, inner and posterior border, there was a distinct purple spot of the size of a small pea. In the centre of this spot there was a ragged opening, that had, in all probability, recently given exit to an unimpregnated germ (*vide* Plate III. fig. 1). An incision was made into the gland, so as to cut through the discoloured portion of it, and it was then remarked that the aperture led into a small cavity, the existence of which is still evident in the wet preparation. The cavity was situated at the very summit of the spot, immediately within the opening on the peritoneal surface; and it was surrounded by a large quantity of what appeared to be extravasated blood (*vide* fig. 2). After the preparation had been immersed in spirit of wine for a few days, I was able to perceive that the spot was made up of four distinct parts;—1st, of an outer vascular layer which surrounded the mass, and extended into it to the depth of $\frac{1}{16}$th of an inch; this layer was gradually thinned as it approached the aperture on the surface of the follicle; and it seemed to consist of *the stroma of the ovary* in a highly congested state. 2nd. Of a thin layer of very dark matter, which appeared to be the remains of *the ovisac*. 3rd. Of a mass of coagulated blood strengthened by a network of intersecting fibres; this mass had the bulk of a hemp-seed, and it was found to be composed of blood-discs, fibrin, and large granular corpuscles; it was, doubtless, therefore, the remains of *the tunica granulosa* in-

* Beweis der von der Begattung unabhängigen periodischen Reifung und Loslösung der Eier, p. 4, quoted by Drs. Baly and Kirkes in their Supplement to the second volume of Müller's Physiology, p. 45.

filtrated with blood. 4thly, and lastly, there was *the cavity* to which I have before alluded.

When the ovaries had become firm and hard by the coagulating action of the spirit, sections were made into them at various parts; by which means a number of yellow bodies (false *corpora lutea*) in different stages of degeneration were brought into view. Some of these bodies were rather large, and one of them contained a well-defined clot, the summit of which communicated with a cicatrix on the surface of the ovary (see the preparation). This circumstance led me to conclude that it was the remains of an old Graafian follicle, from which, at perhaps the last catamenial period, an ovule had escaped.

The Fallopian tubes were highly congested, especially at their fimbriated extremities, where, from the abundance of turgid capillary vessels, the tubes assumed a bright scarlet appearance. The cavities of the oviducts were filled with, and much distended by, a thick bloody mucus, which readily escaped from their peritoneal apertures when the tubes were subjected to slight pressure between the finger and thumb. Both of the Fallopian tubes were carefully laid open by means of a pair of fine scissors—the operation being conducted on a clean white plate, containing a little water,—and their contents were minutely examined. The right tube did not present any object worthy of notice; but the left one contained, at about 1 inch from its distal extremity, a small white vesicular-looking body, which on being floated out into the water, was found to be rather ragged on its surface, and to have the size of the cavity noticed in the recently ruptured Graafian follicle. This body was submitted to microscopical examination. When viewed as an opake object, nothing could be made out beyond the fact that it was covered with white flocculi. It was then placed between two pieces of glass, and examined by the aid of transmitted light; but it was too opake for the eye to distinguish its structure, notwithstanding that the flocculi were very translucent and were seen to be made up of oval nucleated cells (see fig. 6). By the employment of slight pressure the body was readily crushed, and then I could perceive that it was composed of a mass of nucleated cells, among which, at one part, there was a number of highly refractive oil-globules (see fig. 5). The result of this investigation led me to think that the body in question was an ovule, the elements of which had been so far disarranged by the pressure, that the *membrana granulosa* and *yelk-globules* were the only recognisable constituents of it.

The fluid contained in the uterus and Fallopian tubes were likewise subjected to microscopical examination. That removed from the former was found to consist of numerous blood-discs, most of which were strongly beaded at their edges; of much cylindrical epithelium, some of which was distinctly ciliated; of a large quantity of granular corpuscles, like exudation cells; of a few white globules, similar to those found in blood, many of which had apparently passed into the form of spindle-shaped or fusiform bodies by the elongation of their opposite ends; and of a thick gelatinous fluid which united all the elements together (see fig. 3). That from the latter, namely,

the Fallopian tubes, was much the same as the preceding, excepting that the number of the blood-discs was considerably less; that there was a greater abundance of ciliated epithelium; and that the fluid in which the elements floated was not gelatinous, but serous (see fig. 4).

A consideration of the facts thus presented to notice, led me to conclude that the girl had died at the very onset of a catamenial period, for I could not discover any evidence of the occurrence of an external flow; in fact, the secretion found in the vagina was not very abundant, and it had acquired only a pale rose tint.

On instituting a further inquiry into the case, I ascertained that the periodical flux had taken place exactly one week before the woman made the attempt on her life; and with regard to the subsequent history of the case, it may be said that she was laid up with the wound in her throat for a period of twenty-four days before her death, nineteen of which were passed in a separate ward of the Hospital, where, in consequence of her very distressing condition, she was closely watched by a female attendant, so that it is hardly possible that sexual intercourse could have been effected during that period of time.

While I was engaged in the investigation of the preceding case, I received from my friend Dr. Parker of Finsbury Square, who had assisted me in the foregoing inquiry, another uterus and its appendages, which he had removed from the body of a lunatic aged twenty-three. This girl had died, and was examined in St. Luke's Hospital, where she had been a patient for eleven months, under circumstances which deprived her of the opportunity of associating with a male for a long period before her death.

The information obtained by inquiries of the attendant at St. Luke's, as well as by an examination of the organs themselves, led me to conclude that the girl had quitted life during the catamenial period; for the pelvic viscera were much congested, the uterus was considerably enlarged, its vessels were turgid, and its cavity contained a red jelly-like matter; besides which, the Fallopian tubes were filled with a thick muco-sanguineous secretion, and the right ovary presented a dark livid spot on its outer and lower part: many cicatrices were also found on the surfaces of both the ovaries. As in the last case, the livid spot had a hole in its centre; and, on making a section of the ovary so as to divide it through the spot and an adjacent cicatrix, I perceived that the hole led into a cavity, which was surrounded by a deep red tissue, and that the cicatrix communicated with a very perfectly formed *corpus luteum*, having a central cavity containing a dark red clot (see fig. 7 and preparation).

The matter contained in the right Fallopian tube was submitted to careful examination, by which means I discovered a little globular body that had the size of a small pin's head. This body was transferred, as in the last case, to water; then placed between two pieces of glass, and examined under the microscope with a power of 100. The outer constituents of the mass were precisely like those of the preceding; that is, they consisted of nucleated cells, arranged so as to form a shaggy, but tolerably

compact tunic. At one end of the object, near to its surface, there was a transparent ring, enclosing a rather opake granular mass, in which there was an eccentric, highly pellucid spot (see fig. 9). I had no doubt that this was the ovule, consisting of the *zona pellucida*, the *yelk* and the *germinal vesicle*; but, to test the truth of my opinion, I subjected the mass to the action of a little strong acetic acid, by which means the *zona pellucida* was still more clearly brought into view; for it happened that the nucleated cells of the *membrana granulosa* were slightly corrugated by the acid, and, as it were, contracted on their contents. They also acquired a greater transparency; and by using a power of 300, the *zona* presented a distinctly striated appearance, and the granules within it were seen to be highly refractive. Lastly, the whole object was washed with ether, which dissolved away the fat granules, and thus left the *zona* and *vesicle* still more distinct. In the place of the yelk there was now left a somewhat opake structureless material, which seemed to have been the bond of union between the numerous fatty elements of the vitelline mass (see fig. 10).

The fluid matters contained in the uterus and Fallopian tubes, were identical in their physical characters with those noticed in the last case; and the materials composing the recently ruptured Graafian follicle were likewise found to consist of blood-discs, fibrin and large nucleated cells (see fig. 11). The yellow tissue of the *corpus luteum* was made up of a fibro-cellular stroma, in which there was enclosed a number of large granular corpuscles, some of which contained oil-globules; besides these, there was a liquid fat diffused through the tissue, which appeared either as minute particles, or as large elongated highly refractive globules that had been produced by the union of the smaller elements (see fig. 10).

Remarks.—An examination of the preceding facts will, I think, justify me in coming to the following conclusions; namely, *that ovules escape from the ovaries of women during the catamenial flux; and that the escape of these bodies is spontaneous*, id est, *that their extrusion takes place independently of sexual intercourse.* In these respects, the phenomena witnessed are perfectly analogous with those observed by Bischoff, Raciborski, Pouchet and others, while they were instituting inquiries into the changes which occur in the ovaries of other mammals during the periods of heat and rut; and they are also in accordance with a fact long since ascertained by physiologists, namely, that there is a periodical maturation and spontaneous extrusion of ovules from the ovaries of animals still lower in the scale of creation. Hitherto, however, these several circumstances have been regarded only in the light of probable analogies; but I am of opinion that the facts detailed in this paper are quite sufficient to identify the phenomena in question, and also to establish the truth of a great part of the law enunciated by Bischoff, that is, that the ovules formed in the ovaries of females of the human species, and of mammiferous animals, undergo a periodical maturation, quite independently of the influence of the male seminal fluid; and that at the periods of menstruation in the one, and heat in the other, the ova which have become mature disengage themselves from the ovary, and are extruded

whether there be access of the male or no. But admitting that this law is in great part the expression of truth, we have yet to determine whether in human females, as in the females of other animals, the maturation and escape of ovules take place only at the fixed periods mentioned, or whether the discharge of these bodies is always occurring, thus rendering the human female susceptible of impregnation at all times. The final solution of these questions, together with that of the problem concerning the length of time that the ovule retains its faculty of being fecundated after it has quitted the ovary, is of great interest to the physiologist, the obstetric physician, and the medical jurist; for it would not only enable them to pronounce with certainty at what time conception usually takes place, but it would also furnish a starting-point for the determination of other equally important problems, viz. the ordinary, and the most extended periods of human gestation. It must, however, be evident that a great number of independent observations have yet to be made, before any hypothesis relating to this part of the subject can take its place amongst the well-recognised doctrines of science; and, believing that each observation must have some weight and value, I have been led to record the results of my own inquiries, in order that they may be placed in juxtaposition with the facts already elicited by more able investigators. Here, perhaps, I may be allowed to remark, that in addition to the experiments and observations so admirably reviewed by Drs. Baly and Kirkes in their Supplement to the second volume of Professor Müller's Physiology, two cases have been published, in which the phenomena witnessed are in many respects very similar to those noticed by myself. In one of these cases, the girl died shortly after menstruation; and the reporter, M. Janzer, states that, in making an examination of her body, he found on the surface of the left ovary a dark red spot, which had a fissure in its centre. Judging from all the circumstances of the case, Janzer was led to conclude that the spot in question was a recently ruptured Graafian follicle gorged with blood. He instituted a diligent search for the liberated ovule, but he failed to discover it*. The second case is recorded by M. Locatelli†; and he informs us that the woman who was the subject of his investigations died from the effects of an operation made for the relief of an imperforate hymen. He noticed that on one of the ovaries there was a livid spot, in the centre of which there was an aperture. The parts were carefully examined, and the author believes that the spot seen in the ovary was a recently ruptured follicle filled with blood. As in the last case, however, the liberated ovule escaped detection.

* Heidelberger Annalen, Bd. xiii. p. 601–604.

† Frorieps Neue Notizen, Bd. vii. p. 348–350.

Explanation of the Plate.

PLATE III.

Fig. 1. *The uterus and its appendages* as seen from behind. The uterus is laid open by a longitudinal incision in order to show the contained menstrual fluid. The Fallopian tube is highly congested, and its cavity is much distended with sanguinolent mucus. The ovary presents several cicatrices on its surface; and at the inner and lower margin it exhibits a purple spot, in the centre of which there is a small opening through which the ovule had recently escaped.

(*a*.) The spot at which the ovule was found.

Fig. 2. *The left ovary* laid open in a line through the centre of the purple spot, showing the cavity from which the ovule had escaped, surrounded by a clot of dark blood, most of which was effused into the substance of the Graafian follicle.

Fig. 3. *A portion of the sanguinolent matter taken from the uterus*, and magnified about 200 times. It was found to consist of blood-discs, most of which were beaded at the edges (*a a*); of ciliated and cylindrical epithelium (*b b*); of granular corpuscles, some of which were oval and others round (*c c*); of spindle-shaped bodies (*d d*); of numerous minute granules; and of a thick gelatinous fluid which bound all the elements together.

Fig. 4. *Fluid from the Fallopian tube*, treated in like manner: it consisted of nearly the same elements. The fluid in which the corpuscles floated was, however, of a serous, not a gelatinous character.

N.B. To prevent obscurity, only a few of each of the elements have been figured.

Fig. 5. *A portion of the ovule mass* taken from the left Fallopian tube, and magnified 200 times. The entire mass consisted of nucleated cells (*membrana granulosa*) arranged in an irregular manner. Interspersed through the cells were many oil-globules, probably the broken-down yelk.

Fig. 6. *A part of the middle of the mass*, magnified still more, showing the arrangement of the cells and the position of the oil-globules.

Fig. 7. *The right ovary* removed from the uterus and placed vertically.

(*a*.) The external surface showing several stellate cicatrices; and at its

outer and lower part it also exhibits a purple spot, in which there is an aperture from which the ovule has escaped.

(*b*.) The same ovary divided longitudinally through the cicatrix and spot, by which means two false *corpora lutea*, and the ruptured Graafian vesicle were brought into view.

Fig. 8. *A part of the shaggy surface of the ovule*, magnified 200 times, showing the arrangement of the nucleated cells which compose it.

Fig. 9. *The ovule* crushed between glass and magnified 100 times: exhibiting the cellular envelope (*a*); the transparent zona pellucida (*b*); the yelk (*c*); and the germinal vesicle (*d*).

Fig. 10. *The same*, after having been treated with acetic acid and then with ether; magnified 300 times.

Fig. 11. *A portion of the Graafian follicle* from which the ovule has just escaped, magnified 200 times: (*a*) a clot containing blood-discs, fibrin and large nucleated cells; (*b*) some of the cells disengaged.

Fig. 12. *A portion of a corpus luteum*, magnified 200 times; consisting of small fat-globules (*a a*); large nucleated cells containing fat-globules (*b b*); (*c c*) a part of the tissue showing a structure composed of fusiform or fibrous cells; and (*d d*) large fat-globules formed by the aggregation of smaller ones; at *f f* they have assumed other forms, in consequence of their having been pressed between the glass.

V. *On the Air-Engine.* By James Prescott Joule, *F.R.S., F.C.S., Corr. Mem. R.A. Turin, Sec. Lit. and Phil. Soc. Manchester, &c.*

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It has long been suspected that important advantages might be derived from the substitution of air for steam as a prime mover of machinery. It has been alleged that the air-engine would be safer, lighter, and more economical in the expenditure of fuel than the steam-engine. Until comparatively recent times, however, experimental science was hardly in the state of advancement requisite to enable the physicist, in his investigation of this important subject, to arrive at conclusions sufficiently certain to give confidence to the practical machinist. Professor Thomson, Mr. Rankine, and M. Clausius have of late, however, published papers of great value on the mechanical action of gases, and particularly of steam, founded on tolerably correct experimental data. I hope that the following remarks founded on the same general principles, but applied to a particular kind of air-engine, may be interesting to the Royal Society.

The air-engine, the performance of which I propose to discuss, consists of two parts, in one of which the air is compressed into a receiver, where its elasticity is increased by the application of heat, and in the other it is allowed to escape again from the receiver into the atmosphere. By the former work is absorbed, by the latter it is evolved in a larger quantity, the excess constituting the work evolved by the engine on the whole. The simple question, therefore, is to determine the quantity of work so evolved, together with the heat applied to increase the elasticity of the air in the receiver.

In Plate VI. fig. 1 let A be the pump by which air is forced into the receiver C, where heat may be communicated to it from an external source, and B the cylinder, by which the same quantity is allowed to escape again into the atmosphere. Moreover, let the material of which the apparatus is made, with the exception of that part through which heat may be communicated to the air in C, be impervious to, and destitute of capacity for heat. Such a machine may be conceived to work in the following manner.

The cylinder of the pump A being filled with air of the atmospheric temperature and pressure, the piston compresses the air until, at a point *n*, its pressure is rendered equal to that of the air in the receiver C, which has been previously filled with air of an elevated temperature and pressure. The work absorbed by this action will be that communicated to the air in the cylinder, minus the work due to the atmospheric

pressure through mn. The moment the piston has passed the point n the valve will open, admitting the air into the receiver C; and as this receiver may be conceived to be of indefinite magnitude, the alteration of pressure in it, consequent upon the introduction of fresh air, may be neglected. Heat is then communicated to the air in the receiver, in order to restore its temperature to the intensity which existed before the admission of air at a lower temperature. The air is then allowed to escape from the receiver into the base of the cylinder B, evolving work until, on the arrival of the piston at n', the same quantity has been removed from the receiver as was forced into it by the pump. The further supply of air from the receiver is then cut off, and that which has entered the cylinder expands, evolving work until, on the arrival of the piston at m', its pressure is reduced to that of the atmosphere. By opening valves at the bases of A and B, the pistons are then brought to their first positions.

The problem which must be solved in order to estimate the power and consumption of fuel in an engine similar to that just described, is as follows:—To determine the pressure and temperature for any point of the stroke of a piston which compresses a given volume of air, and the quantity of work absorbed in forcing the piston to that point. For the temperature and pressure Poisson has furnished the following formulæ,—

$$\frac{T'}{T}=\left(\frac{V}{V'}\right)^{k-1},$$

and

$$\frac{P'}{P}=\left(\frac{V}{V'}\right)^{k},$$

where T, P, and V are the temperature from absolute zero (estimated at 491° Fahr. below the freezing-point of water), pressure, and volume of the air before compression; T′, P′, and V′ the temperature from absolute zero, pressure, and volume of air after compression; and k is the ratio of the specific heat of air at constant pressure to that at constant volume. Professor W. Thomson has deduced, as a consequence of the above, the following formula for the work absorbed,

$$W=PV\frac{1}{k-1}\left\{\left(\frac{V}{V'}\right)^{k-1}-1\right\}*.$$

From the foregoing formulæ I have calculated the work absorbed by compressing air in a cylinder 1 foot long, and of the capacity of 12 cubic inches, the absolute temperature of the air, and its pressure at each tenth of an inch of the piston's progress. The following data were employed in the computation:—Weight of 100 cubic inches of atmospheric air of 15 lbs. pressure on the square inch, and 491°

* The above formula was kindly communicated to the author by Professor Thomson, in a letter dated January 15, 1851, from which the following is an extract:—"It is required to find the work necessary to compress a given mass of air to a given fraction of its volume, when no heat is permitted to leave the air. Let P, V, T be the primitive pressure, volume, and temperature, respectively; let p, v, and t be the pressure, volume, and temperature at any instant during the compression; and let P′, V′, and T′ be what they become

Fahr. from the absolute zero, 33·2237 grs.; specific heat of air at constant volume, 0·19742. Ratio of the specific heat of air at constant pressure to that at constant volume, as determined from the experiments of Delaroche and Berard, and the mechanical equivalent of heat, 1·3519325*. The results are shown in Table I.

I now proceed to give some estimates of the performance of an air-engine similar in principle to that already described, worked at various pressures and temperatures, those of the atmospheric air being 15 lbs. on the square inch, and 32° Fahr. or 491° Fahr. from the absolute zero. In order to render the results easily available in calculating the duty of engines of greater size, I shall assume that the condensing pump is 12 inches long, and has a sectional area equal to 1 square inch, and that the cylinder, also of 1 inch section, has a length which may be made to vary according to the pressure and temperature employed.

I take as the first example, a case in which the receiver C contains air of the atmospheric density, and of which the absolute temperature is 849°·464 Fahr. or 390°·464 of the scale of Fahrenheit's thermometer. The pressure in the receiver will then be 25·95104 lbs. on the square inch, as given in the third column of Table II. The air in the pump A will be brought to the same pressure, and to the absolute temperature 566°·3094 after the piston has traversed 4 inches. The work absorbed by the air will be 6·537154 foot-pounds, from which, by subtracting 5 foot-pounds, the work communicated by the pressure of the atmosphere following the piston, we obtain 1·537154 foot-pounds as the work of the engine absorbed by the first part of the stroke. This result is consigned to column 6. Immediately after the piston has passed the fourth inch of the pump, the valve will be opened admitting the compressed air into the receiver C. The work of the engine absorbed by the re-

when the compression is concluded. Then if k denote the ratio of the specific heat of air at constant pressure to the specific heat of air kept in a space of constant volume, and if, as appears to be nearly, if not rigorously true, k be constant for varying temperatures and pressures, we shall have by the investigation in Miller's 'Hydrostatics' (Edit. 1835, p. 22)—

$$\frac{1+Et}{1+ET}=\left(\frac{V}{v}\right)^{k-1}.$$

But

$$\frac{pv}{PV}=\frac{1+Et}{1+ET},$$

therefore

$$pv=PV\left(\frac{V}{v}\right)^{k-1}.$$

Now the work done in compressing the mass from volume v to volume $v-dv$ will be pdv, or by what precedes,

$$PV.V^{k-1}\frac{dv}{v^k}.$$

Hence by the integral calculus we readily find, for the work, W, necessary to compress from V to V′,

$$W=PV.\frac{1}{k-1}\left\{\left(\frac{V}{V'}\right)^{k-1}-1\right\}."$$

* The experiments of Desormes and Clement give 1·354; those of Gay-Lussac and Welter 1·375; and those described under the article 'Hygrometry' (Enc. Brit.), 1·333. See Art. 'Sound,' Enc. Brit., 7th Edit.

maining 8 inches of the piston's stroke will be $\frac{8}{12}(25{\cdot}95104-15)=7{\cdot}300693$ foot-pounds, as given in the seventh column. The air thus forced into the receiver at the absolute temperature 566°·3094 FAHR. must then be raised to 849°·464 FAHR., the constant absolute temperature of the receiver. The heat necessary for this purpose, being that due to the capacity for heat of air at constant pressure, will be that which is able to raise the temperature of 1 lb. of water 0°·04304312 FAHR., as given in column 15. On leaving the receiver, the air enters the cylinder of expansion B, and having propelled the piston through 12 inches, the same quantity of air will have passed out of the receiver as was pumped into it by A. The further supply of air is then cut off, and the air after expanding through the remaining 6 inches of the cylinder (which in this case must be 18 inches long), will be reduced to the pressure of 15 lbs. on the square inch, and the absolute temperature $\frac{3}{2}(491°)=736°{\cdot}5$. The work evolved by the piston will also be to that absorbed in the condensing pump, as the volume of the cylinder B is to that of the pump A; from which we find $\frac{3}{2}(7{\cdot}300693)=10{\cdot}95104$ foot-pounds, and $\frac{3}{2}(1{\cdot}537154)=2{\cdot}305731$ foot-pounds, the work evolved by the first and second parts of the piston's stroke, as given in columns 11 and 12. The work evolved by the engine on the whole, being the difference between the work evolved by B, and the work absorbed by A, will be equal to one-third of the former, or one-half of the latter, or 4·418924 foot-pounds, as given in column 14. Dividing this by 0°·04304312, we obtain 102·66276 foot-pounds as the work evolved by the engine out of each 1° FAHR. per lb. of water communicated to the receiver. This result, which is consigned to the sixteenth column, informs us of the economical value of the engine, which is of course great in proportion to its approach to 772 foot-pounds, the theoretical maximum. The seventeenth column contains the theoretical duty according to Professor THOMSON's law, viz. that the range of temperature divided by the maximum absolute temperature is equal to the fraction of heat converted into force by any perfect engine*.

It will be observed that the numbers in column 16, representing the work evolved out of each unit of heat, increase with the temperature and pressure of the air in the receiver. In every example given, with the exception of the first, the economical value of the air-engine in question is greater than that of the steam-engine calculated by Mr. RANKINE in his paper on the Mechanical Action of Heat†. In considering the relative merits of the engines, we must not, however, lose sight of a most important fact discovered by RANKINE and CLAUSIUS, viz. that a portion of the heat

* See Professor THOMSON's "Investigation of the Duty of a perfect Thermo-Dynamic Engine," at the end of this paper.

† Transactions of the Royal Society of Edinburgh, vol. xx. part 1. Professor THOMSON, in a paper "On the Dynamical Theory of Heat," recently read before the Royal Society, Edinburgh, gives 209 foot-pounds as the duty of an absolutely perfect steam-engine, with a range of temperature between 30° and 140° Centigrade.

Table I.

Distance traversed by piston, in inches.	Work absorbed, in foot-pounds.	Temperature from absolute zero, in degrees Fahr.	Pressure on the piston, in lbs.	Distance traversed by piston, in inches.	Work absorbed, in foot-pounds.	Temperature from absolute zero, in degrees Fahr.	Pressure on the piston, in lbs.
0	0·	491	15	6·0	11·77479	626·6480	38·28805
0·1	0·1257008	492·4481	15·17066	6·1	12·09828	630·3747	39·16857
0·2	0·2528426	493·9128	15·34473	6·2	12·42768	634·1694	40·08375
0·3	0·3814514	495·3944	15·52230	6·3	12·76566	638·0630	41·03738
0·4	0·5113882	496·8913	15·70343	6·4	13·11172	642·0498	42·03120
0·5	0·6432696	498·4106	15·88841	6·5	13·46626	646·1341	43·06763
0·6	0·7763749	499·9440	16·07709	6·6	13·82962	650·3201	44·14936
0·7	0·9111464	501·4966	16·26974	6·7	14·20221	654·6124	45·27926
0·8	1·047533	503·0678	16·46643	6·8	14·58441	659·0155	46·46043
0·9	1·185586	504·6582	16·66731	6·9	14·97668	663·5345	47·69625
1·0	1·325341	506·2682	16·87248	7·0	15·37948	668·1749	48·99042
1·1	1·466805	507·8979	17·08209	7·1	15·79334	672·9426	50·34692
1·2	1·610032	509·5479	17·29627	7·2	16·21879	677·8438	51·77015
1·3	1·755090	511·2190	17·51516	7·3	16·65633	682·8845	53·26481
1·4	1·901962	512·9110	17·73892	7·4	17·10672	688·0730	54·83624
1·5	2·050727	514·6248	17·96770	7·5	17·57047	693·4156	56·49008
1·6	2·201444	516·3611	18·20167	7·6	18·04842	698·9216	58·23268
1·7	2·354089	518·1196	18·44097	7·7	18·54128	704·5995	60·07098
1·8	2·508791	519·9018	18·68582	7·8	19·04988	710·4585	62·01267
1·9	2·665542	521·7076	18·93638	7·9	19·57510	716·5093	64·06620
2·0	2·824402	523·5377	19·19283	8·0	20·11797	722·7632	66·24102
2·1	2·985424	525·3927	19·45538	8·1	20·67947	729·2318	68·54755
2·2	3·148667	527·2733	19·72426	8·2	21·26078	735·9287	70·99750
2·3	3·314184	529·1801	19·99966	8·3	21·86317	742·8683	73·60395
2·4	3·481993	531·1133	20·28182	8·4	22·48795	750·0659	76·38145
2·5	3·652224	533·0744	20·57099	8·5	23·13667	757·5392	79·34657
2·6	3·824870	535·0633	20·86740	8·6	23·81096	765·3072	82·51786
2·7	4·000022	537·0811	21·17132	8·7	24·51263	773·3907	85·91642
2·8	4·177719	539·1282	21·48302	8·8	25·24353	781·8109	89·56590
2·9	4·358080	541·2060	21·80279	8·9	26·00607	790·5954	93·49398
3·0	4·541123	543·3147	22·13095	9·0	26·80260	799·7716	97·73172
3·1	4·726945	545·4554	22·46778	9·1	27·63584	809·3707	102·3153
3·2	4·915606	547·6288	22·81364	9·2	28·50889	819·4284	107·2862
3·3	5·107206	549·8361	23·16887	9·3	29·42512	829·9836	112·6929
3·4	5·301779	552·0776	23·53383	9·4	30·38842	841·0810	118·5920
3·5	5·499456	554·3549	23·90892	9·5	31·40316	852·7710	125·0499
3·6	5·700287	556·6685	24·29452	9·6	32·47432	865·1110	132·1452
3·7	5·904370	559·0196	24·69107	9·7	33·60755	878·1660	139·9716
3·8	6·111816	561·4094	25·09902	9·8	34·80946	892·0122	148·6412
3·9	6·322706	563·8389	25·51884	9·9	36·08755	906·7362	158·2897
4·0	6·537154	566·3094	25·95104	10·0	37·45073	922·4402	169·0827
4·1	6·755242	568·8218	26·39613	10·1	38·90927	939·2430	181·2239
4·2	6·977122	571·3779	26·85467	10·2	40·47547	957·2860	194·9666
4·3	7·202863	573·9785	27·32725	10·3	42·16396	976·7377	210·6300
4·4	7·432590	576·6250	27·81448	10·4	43·99234	997·8010	228·6204
4·5	7·666465	579·3193	28·31703	10·5	45·98215	1020·724	249·4640
4·6	7·904577	582·0624	28·83559	10·6	48·15980	1045·811	273·8522
4·7	8·147090	584·8562	29·37090	10·7	50·55854	1073·445	302·7106
4·8	8·394129	587·7021	29·92373	10·8	53·22073	1104·114	337·3058
4·9	8·645876	590·6023	30·49494	10·9	56·20106	1138·448	379·4122
5·0	8·902414	593·5577	31·08536	11·0	59·57200	1177·282	431·5900
5·1	9·164006	596·5713	31·69598	11·1	63·43234	1221·754	497·6597
5·2	9·430732	599·6440	32·32776	11·2	67·92089	1273·463	583·5623
5·3	9·702832	602·7787	32·98178	11·3	73·23965	1334·736	699·0180
5·4	9·980470	605·9771	33·65917	11·4	79·69880	1409·147	860·9854
5·5	10·26387	609·2420	34·36112	11·5	87·80466	1502·528	1101·650
5·6	10·55323	612·5754	35·08897	11·6	98·46010	1625·281	1489·565
5·7	10·84876	615·9800	35·84405	11·7	113·4919	1798·450	2197·699
5·8	11·15067	619·4581	36·62784	11·8	137·4363	2074·295	3802·170
5·9	11·45928	623·0133	37·44196	11·9	187·1806	2647·359	9705·187

TABLE II.

No. of Example.	Receiver C.			Pump of Compression A. Length 12 inches. Sectional Area = 1 square inch.				Cylinder of Expansion B. Sectional Area = 1 square inch.								
	Density of the air, that of the atmosphere being called unity.	Pressure of the air in lbs. on the square inch.	Absolute temperature of the air, in degrees FAHR. from the absolute zero.	Length of the first part of the stroke.	Work of the engine absorbed by the first part of the stroke of the piston, in foot-pounds.	Work of the engine absorbed by the second part of the stroke of the piston, in foot-pounds.	Absolute temperature of the air forced into receiver C, in degrees FAHR. from the absolute zero.	Length of cylinder B, in inches.	Length of the first part of the piston's stroke, in inches.	Work communicated to the engine by the first part of the stroke of the piston, in foot-pounds.	Work communicated to the engine by the second part of the stroke, in foot-pounds.	Absolute temperature of the air escaping into the atmosphere, in degrees FAHR. from absolute zero.	Work evolved by the engine by each stroke of the piston, in foot-pounds.	Heat communicated to the air in receiver C, in degrees FAHR. per capacity of a lb. of water.	Work evolved out of each degree FAHR. in the capacity of a lb. of water, in foot-pounds.	Difference between the numbers in columns 4 and 13, divided by the numbers in column 4, and multiplied by the mechanical equivalent of heat.
1	1	25·95104	849·464	4	1·537154	7·300693	566·3094	18	12	10·95104	2·305731	736·5	4·418924	0·04304312	102·6628	102·6626
2	1	66·24102	2168·289	8	10·11797	17·08034	722·7632	36	12	51·24102	30·35391	1473·0	54·39662	0·2197384	247·5517	247·5515
3	2	66·24102	1084·145	8	10·11797	17·08034	722·7632	18	6	25·62051	15·17695	736·5	13·59915	0·0549346	247·5517	247·5515
4	2	169·0827	2767·321	10	24·95073	25·68045	922·4402	36	6	77·04135	74·85219	1473·0	101·2624	0·2804454	361·0769	361·0770
5	4	169·0827	1383·660	10	24·95073	25·68045	922·4402	18	3	38·52067	37·42610	736·5	25·31569	0·07011137	361·0769	361·0770
6	8	431·59	1765·923	11	45·82200	34·71583	1177·282	18	1·5	52·07375	68·73300	736·5	40·26892	0·08948094	450·0278	450·0279
7	20	1101·65	1803·034	11·5	73·42966	45·27708	1502·528	14·4	0·6	54·33250	88·11559	589·2	23·74135	0·04568073	519·7236	519·7238
8	100	9705·187	3176·831	11·9	172·3056	80·75156	2647·359	14·4	0·12	96·90187	206·7667	589·2	50·61143	0·0804865	628·8189	628·8189
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17

employed to evaporate water in the boiler is afterwards evolved in the form of work, in consequence of the liquefaction, in the cylinder, of a portion of the expanding vapour. This fact would induce the hope that a great portion of the latent heat of evaporation, which is at present almost entirely lost, might by an increase of temperature, and by extending the principle of expansion, be converted into mechanical effect.

If, as would appear from the experiments of De la Rive and Marcet, Haycraft and Dulong, the capacity for heat of a given volume is the same in all gases taken at the same pressure and temperature, the results of the above Tables will be equally true whatever elastic fluid be employed.

It now only remains to offer a few observations, with a view to facilitate the labours of those who may be desirous of constructing a good practical air-engine.

It may be remarked, in the first place, that the receiver C need not be of much greater capacity than the cylinder B. For in the reciprocating engine, the air could be introduced from the pump A, at the same time that an equal amount would be expelled into the cylinder B. It would therefore be only requisite to pass the air through tubes heated by a proper furnace, as in Neilson's *hot-blast*, the tubes themselves constituting the receiver C. For a temperature under the red heat, these tubes might be constructed of wrought or cast iron. They might be either straight, like the tubes of a locomotive boiler, or arranged in the form of a coil, as represented by fig. 2, in which *a* is the pipe which conveys the air from the pump, *c*, *c*, *c*, &c. is the coil of wrought or cast-iron tubing, and *b* is the pipe which conveys the heated air to the cylinder. The coil is surrounded by a massive arch of brickwork, which serves at once to support the pipes, and to prevent waste of heat. To prevent the temperature exceeding the proper limits, the pipe *b* might, as it expands by the heat of the inclosed air, move a piece of mechanism in connection with the damper of the flue. I may remark that, on the scales adopted, fig. 2 represents the size of receiver which would be required for an engine the cylinder of which is 3 feet in diameter.

I would here venture to suggest whether the combustion of the fuel could not, by suitable mechanical arrangements, be carried on within the receiver C; if this could be accomplished, the heat, which in the form of receiver already described is lost up the chimney, would be economized, and a great saving of weight and space would be effected. An engine furnished with a receiver of this kind would be strikingly analogous to the electro-magnetic engine, and present a beautiful illustration of the evolution of mechanical effect from chemical forces.

In both of the above forms of receiver, it would be desirable, as already hinted, that the introduction of the air into the receiver should be simultaneous with the expulsion of the same quantity into the cylinder. This is necessary in order both to keep the pressure in the receiver uniform and to promote the smooth action of the engine. For this purpose the piston-rods of the pump and cylinder, *a* and *b* (fig. 3), must be attached to cranks on different parts of the circumference of the revolving shaft *c c*, so contrived that the piston shall arrive at the top or bottom of the cylinder

the moment that the pump-valve opens admitting a fresh supply of air into the receiver. The cylinder should of course be provided with proper expansion gear to cut off the air at the required part of the stroke, which must be a constant quantity for each engine. The valves of the pump would of course be self-acting.

In an engine similar to that described, it will be obvious that if the temperature of the receiver be kept constant, the pressure of air in it will also remain constant. For whilst the same quantity of air is always introduced into the receiver by each stroke of the pump, the quantity expelled out of it would increase with an augmentation and decrease with a diminution of pressure.

In conclusion, I would recommend the examples No. 3 and No. 5 of Table II. to the attention of those who may be willing to construct an air-engine. In both of these cases the capacity of the pump is two-thirds of that of the cylinder. In the cylinder of No. 3 the air is to be cut off at one-third of the stroke; and in that of No. 5 at one-sixth of the stroke. The temperature of the air in the receiver (supposing that of the atmosphere to be 32° FAHR.) is 625°·145 FAHR. in No. 3, and 924°·66 FAHR. in No. 5. The consumption of fuel in No. 3 need not exceed one-half, nor that in No. 5 one-third of that in the most perfect steam-engines at present constructed.

Acton Square, Salford, Manchester,
May 6, 1851.

Note to the foregoing Paper, with a New Experimental Determination of the Specific Heat of Atmospheric Air.

Received March 23, 1852.

Since the above was written, Professor W. H. MILLER has directed my attention to the probable incorrectness of the value of k, as deduced from the experiments of DELAROCHE and BERARD on the specific heat of air, and my own determination of the mechanical equivalent of heat; in comparison with the value deduced from the numerous and excellent experiments on the velocity of sound. Mr. RANKINE considers that the discrepancy between the two values arises from the incorrectness of DELAROCHE and BERARD's result, an opinion which would seem to be justified by the entire want of accordance between the determination of these philosophers, and those of SUERMANN, and CLEMENT and DESORMES. I have therefore been induced to make the following careful experiments in order to obtain a fresh and, if possible, more correct value of the specific heat of air at constant pressure.

The apparatus I employed is represented by fig. 4, in which a and b are two vessels, each of which contains a coil of leaden piping, eight yards long and one quarter of an inch in internal diameter. The coil of the upper vessel passes three-eighths of an inch through the bottom, to which it is soldered at c, and is thence connected with the coil of the lower vessel by a piece of vulcanized india rubber tubing. This part

of the apparatus will be better understood by a reference to fig. 5, in which a section of it is represented, *a* being the upper, *b* the lower vessel, and *w* the surface of the water in the latter. *x x* are a pair of wooden pincers by means of which the india rubber tube could be compressed so as to prevent, when desired, any communication between the air in the two coils of piping. Referring again to fig. 4, *g* is a gas-lamp to maintain the water in the upper vessel at a constant high temperature, and *j* is a tall jar filled with coarsely pounded chloride of calcium, in passing through which the air was entirely deprived of aqueous vapour; a length of vulcanized india rubber tubing, *p*, connects the coil of the lower vessel with a good air-pump, each barrel of which was found to have a capacity of 12·77 cubic inches. The temperature of the pump could be ascertained by means of a small thermometer, the bulb of which was kept in contact with one of the barrels.

The method of experimenting was as follows:—The lower vessel being filled with cold water, and the upper with water raised to about 190°, their exact temperatures were read off, with the usual precautions, from the scales of delicate and accurate thermometers. The pump was then worked at a uniform velocity for twenty-six minutes, the water in the lower vessel being agitated from time to time by a stirrer. The examination of the barometer and thermometers a second time occupied four minutes more; so that the whole time occupied by each experiment was exactly half an hour. The pincers were now applied so as to cut off all communication between the air in the two coils, and the effect of the various causes of a change of temperature in the lower vessel, unconnected with the current of heated air, was observed during another half-hour. Experiments of both the above kinds were repeated several times with the results tabulated below.

I may remark in this place that I had ascertained, by preliminary experiments, that the air passed from the coils of the vessels sensibly at the temperatures registered by the thermometers plunged into the surrounding water.

SERIES I.—Pump worked 26′, at the rate of twenty-four strokes per minute.

No. of Experiment.	Source of calorific effect.	Height of Barometer.	Temperature of Barometer.	Temperature of Air-pump.	Temperature of upper vessel.	Temperature of the room.	Temperature of the lower vessel.		Increase of temperature.
							Commencement of Experiment.	Termination of Experiment.	
			°	°	°	°	°	°	°
1	Radiation					46·081	41·270	41·814	0·544
1	Heated air and radiation	30·195	46	49·3	189·28	46·188	41·814	42·802	0·988
2	Radiation					46·497	42·802	43·304	0·502
2	Heated air and radiation	30·205	46·75	50·3	189·43	46·785	43·304	44·246	0·942
3	Radiation					46·948	44·246	44·694	0·448
3	Heated air and radiation	30·22	47·5	51·1	189·89	47·068	44·694	45·590	0·896
4	Radiation					47·197	45·590	45·983	0·393
4	Heated air and radiation	30·235	48·	51·7	194·85	47·283	45·983	46·856	0·873
5	Radiation					47·455	46·856	47·211	0·355
Mean.	Heated air and radiation	30·214	47·06	50·6	190·862	46·831	43·949	44·874	0·925
Mean.	Radiation					46·836	44·153	44·601	0·448

It will be observed that the excess of the temperature of the room above the mean temperature of the water in the lower vessel, was, in the experiments with heated air, 2°·42, but in the experiments on the effect of radiation 2°·459. A comparison of the several experiments with one another, furnished the means of determining the amount of the small correction due to this circumstance. Hence $0°{\cdot}925+0°{\cdot}002-0°{\cdot}448=0°{\cdot}479$ will be the corrected mean increase of temperature due to the current of heated air. The material in which this increase took place consisted of 175500 grs. of water, 15635 grs. of copper, and 53370 grs. of lead, the whole having a capacity for heat equivalent to that of 178535 grs. of water. The volume of air passed through the pump was $12{\cdot}77\times26\times24=7968{\cdot}48$ cubic inches, which, at the observed barometric pressure and the temperature 50°·6, would weigh 2537·94 grs. We have therefore for the specific heat of atmospheric air at constant pressure—

$$\frac{178535\times0{\cdot}479}{2537{\cdot}94\times146{\cdot}45}=0{\cdot}23008.$$

Series II.—Pump worked 26′, at the rate of forty strokes per minute.

No. of Experiment.	Source of calorific effect.	Height of Barometer.	Temperature of Barometer.	Temperature of Air-pump.	Temperature of upper vessel.	Temperature of the room.	Temperature of the lower vessel.		Increase of temperature.
							Commencement of Experiment.	Termination of Experiment.	
			°	°	°	°	°	°	°
1	Radiation					47·223	44·200	44·648	0·448
1	Heated air and radiation	30·6	47·75	52	197·71	47·558	44·648	45·902	1·254
2	Radiation					47·841	45·902	46·319	0·417
2	Heated air and radiation	30·602	48·25	53·5	198·63	48·099	46·319	47·516	1·197
3	Radiation					48·339	47·516	47·860	0·344
3	Heated air and radiation	30·61	49·5	55·4	202·42	49·107	49·327	50·443	1·116
4	Radiation					49·524	50·443	50·728	0·285
4	Heated air and radiation	30·607	50·25	56·4	203·13	49·850	50·728	51·809	1·081
5	Radiation					50·030	51·809	52·037	0·228
Mean.	Heated air and radiation	30·605	48·94	54·32	200·472	48·653	47·755	48·917	1·162
Mean.	Radiation					48·591	47·974	48·318	0·344

In the above series $1°{\cdot}162+0°{\cdot}006-0°{\cdot}344=0°{\cdot}824$ will be the corrected mean increase of temperature due to the current of heated air. The material in which this increase took place consisted of 175000 grs. of water, 15635 grs. of copper, and 53370 grs. of lead, the whole having a capacity for heat equivalent to that of 178035 grs. of water. The volume of air passed through the pump was $12{\cdot}77\times26\times40=13280{\cdot}8$ cubic inches, which, at the observed barometric pressure and the temperature 54°·32, would weigh 4252·7 grs. Hence we have for the specific heat—

$$\frac{178035\times0{\cdot}824}{4252{\cdot}7\times152{\cdot}136}=0{\cdot}22674.$$

By another series of experiments, in which the air-pump was worked at the velocity of twenty strokes per minute for twenty minutes, I obtained the value 0·2325. The mean of the three results is 0·22977, or nearly 0·23, which we may take as the specific heat of air at constant pressure determined by the above experiments.

Professor W. H. Miller has remarked that Moll's experiments, when correctly reduced, give a velocity of sound equal to 332·475 metres per second in dry air at 32°. Hence he deduces 1·41029 as the value of k. Calling it in round numbers 1·41, and the mechanical equivalent of heat 772, we obtain 0·238944 as the value of the specific heat of air at constant pressure, a result sufficiently near the experimental determination to show that the value of k, as deduced by Professor Miller, is much nearer the truth than that upon which the tables of the foregoing paper are founded.

The values of k, as determined by the experiments of Desormes and Clement, Gay-Lussac and Welter, and Mr. Meikle, referred to in the note to page 67, are respectively only 1·354, 1·375, and 1·333. In these experiments a small portion of air having been withdrawn from a large receiver, the equilibrium was re-established by opening for an instant a large aperture communicating with the external air, and then, after the receiver and its contents had regained their original temperature, the alteration of pressure, indicating the sudden rise of temperature which had taken place on the admission of the air, was noted. But it is obvious that the sudden admission of the air would cause the development of *sound*, and that, a portion of the *vis viva* escaping in this form, the increase of temperature and the deduced ratio of the specific heats would be diminished accordingly.

I subjoin Tables, similar to Tables I. and II., calculated from the data $k=1·41$, and the specific heat of air at constant volume $=0·169464$, or at constant pressure $=0·238944$.

In Table IV., the examples 9, 10 and 11 may be suggested to the notice of the practical engineer, the temperature of the receiver being in all those cases below that of redness. I may remind the reader that the Table is founded on the supposition that the air which enters the pump has 491° of temperature from the absolute zero, and that its pressure is 15 lbs. on the square inch. If this initial temperature be altered, the whole of the other temperatures in the Table must be altered in the same proportion, but the pressure, work and economical duty will remain unchanged. If the initial pressure be altered, all the other pressures and work will suffer a proportionate change, but the temperatures and economical duty will remain the same. The above are obvious deductions from the formulæ on which the Tables are founded.

Acton Square, Salford,
March 20, 1852.

Table III.

Distance traversed by piston, in inches.	Work absorbed, in foot-pounds.	Temperature from absolute zero, in degrees Fahr.	Pressure on the piston, in pounds avoirdupois.	Distance traversed by piston, in inches.	Work absorbed, in foot-pounds.	Temperature from absolute zero, in degrees Fahr.	Pressure on the piston, in pounds avoirdupois.
0	0	491	15	6·0	12·025096	652·3847	39·86055
0·1	0·1257463	492·6876	15·17803	6·1	12·36122	656·8958	40·81647
0·2	0·2529680	494·3950	15·35970	6·2	12·70547	661·5159	41·81223
0·3	0·3817395	496·1232	15·54513	6·3	13·05820	666·2498	42·85023
0·4	0·5120683	497·8723	15·73438	6·4	13·41977	671·1023	43·93309
0·5	0·6439993	499·6429	15·92769	6·5	13·79055	676·0785	45·06354
0·6	0·7775396	501·4351	16·12503	6·6	14·17096	681·1838	46·24464
0·7	0·9127567	503·2498	16·32661	6·7	14·56146	686·4245	47·47966
0·8	1·049665	505·0872	16·53253	6·8	14·96245	691·8060	48·77217
0·9	1·188301	506·9478	16·74292	6·9	15·37448	697·3358	50·12596
1·0	1·328719	508·8323	16·95793	7·0	15·79807	703·0207	51·54529
1·1	1·471827	510·7529	17·17773	7·1	16·23377	708·8680	53·03472
1·2	1·615031	512·6748	17·40240	7·2	16·68220	714·8862	54·59923
1·3	1·761007	514·6339	17·63216	7·3	17·14397	721·0835	56·24433
1·4	1·908922	516·6190	17·86716	7·4	17·61984	727·4700	57·97600
1·5	2·058809	518·6306	18·10755	7·5	18·11050	734·0550	59·80080
1·6	2·210724	520·6694	18·35353	7·6	18·61680	740·8498	61·72605
1·7	2·364719	522·7361	18·60529	7·7	19·13958	747·8659	63·75969
1·8	2·520828	524·8312	18·86299	7·8	19·67980	755·1160	65·91058
1·9	2·679114	526·9555	19·12686	7·9	20·23844	762·6133	68·18854
2·0	2·839636	529·1098	19·39710	8·0	20·81664	770·3732	70·60445
2·1	3·002421	531·2945	19·67392	8·1	21·41559	778·4115	73·17045
2·2	3·167547	533·5106	19·95757	8·2	22·03660	786·7459	75·90002
2·3	3·335073	535·7589	20·24830	8·3	22·68110	795·3954	78·80838
2·4	3·505041	538·0400	20·54632	8·4	23·35061	804·3808	81·91249
2·5	3·677529	540·3549	20·85193	8·5	24·04690	813·7254	85·23164
2·6	3·852596	542·7044	21·16540	8·6	24·77180	823·4542	88·78743
2·7	4·030240	545·0885	21·48700	8·7	25·52742	833·5950	92·60451
2·8	4·210744	547·5110	21·81705	8·8	26·31602	844·1786	96·71086
2·9	4·393947	549·9697	22·15585	8·9	27·14016	855·2390	101·1385
3·0	4·580211	552·4695	22·50375	9·0	28·00265	866·8144	105·9244
3·1	4·769047	555·0038	22·86105	9·1	28·90667	878·9468	111·1106
3·2	4·961064	557·5808	23·22823	9·2	29·85575	891·6840	116·7460
3·3	5·156197	560·1996	23·60557	9·3	30·85385	905·0792	122·8892
3·4	5·354524	562·8613	23·99352	9·4	31·90550	919·1930	129·6058
3·5	5·556132	565·5670	24·39246	9·5	33·01577	934·0936	136·9750
3·6	5·761092	568·3177	24·80292	9·6	34·19049	949·8591	145·0905
3·7	5·969523	571·1150	25·22533	9·7	35·43632	966·5791	154·0638
3·8	6·181561	573·9607	25·66015	9·8	36·76094	984·3564	164·0289
3·9	6·397243	576·8553	26·10795	9·9	38·17335	1003·312	175·1490
4·0	6·616735	579·8010	26·56929	10·0	39·68387	1023·584	187·6224
4·1	6·840106	582·7988	27·04473	10·1	41·30480	1045·338	201·6945
4·2	7·067466	585·8501	27·53490	10·2	43·05077	1068·770	217·6720
4·3	7·299071	588·9584	28·04045	10·3	44·93904	1094·112	235·9413
4·4	7·534902	592·1234	28·56207	10·4	46·99081	1121·648	256·9966
4·5	7·775119	595·3475	29·10049	10·5	49·23177	1151·723	281·4802
4·6	8·019950	598·6331	29·65651	10·6	51·69402	1184·768	310·2390
4·7	8·269468	601·9818	30·23092	10·7	54·41741	1221·318	344·4105
4·8	8·523854	605·3958	30·82462	10·8	57·45355	1262·065	385·5594
4·9	8·783274	608·8774	31·43856	10·9	60·86887	1307·901	435·8856
5·0	9·047890	612·4287	32·07366	11·0	64·75245	1360·021	498·5821
5·1	9·317890	616·0523	32·73102	11·1	69·22596	1420·059	578·4345
5·2	9·593477	619·7509	33·41175	11·2	74·46110	1490·318	682·9350
5·3	9·874827	623·5267	34·11703	11·3	80·71017	1574·184	824·4195
5·4	10·16216	627·3830	34·84815	11·4	88·36276	1676·887	1024·575
5·5	10·45581	631·3240	35·60646	11·5	98·06077	1807·041	1324·918
5·6	10·75570	635·3486	36·39342	11·6	110·9605	1980·164	1814·815
5·7	11·06235	639·4641	37·21060	11·7	129·4314	2228·056	2722·671
5·8	11·37594	643·6727	38·05962	11·8	159·4567	2631·016	4822·635
5·9	11·69678	647·9786	38·94231	11·9	223·8930	3495·794	12815·505

TABLE IV.

No. of Example.	Receiver C.			Pump of Compression A. Length 12 inches. Sectional Area = 1 square inch.				Cylinder of Expansion B. Sectional Area = 1 square inch.					Work evolved by the engine by each stroke of the piston, in foot-pounds.	Heat communicated to the air in receiver C, in degrees FAHR. per capacity of a lb. of water.	Work evolved out of each degree FAHR. in the capacity of a lb. of water, in foot-pounds.	Difference between the numbers in columns 4 and 13, divided by the numbers in column 4, and multiplied by the mechanical equivalent of heat.
	Density of the air, that of the atmosphere being called unity.	Pressure of the air in pounds on the square inch.	Absolute temperature of the air, in degrees FAHR. from the absolute zero.	Length of the first part of the stroke.	Work of the engine absorbed by the first part of the stroke of the piston, in foot-pounds.	Work of the engine absorbed by the second part of the stroke of the piston, in foot-pounds.	Absolute temperature of the air forced into the receiver C, in degrees FAHR. from the absolute zero.	Length of the cylinder B, in inches.	Length of the first part of the piston's stroke, in inches.	Work communicated to the engine by the first part of the stroke, in foot-pounds.	Work communicated to the engine by the second part of the stroke, in foot-pounds.	Absolute temperature of the air escaping into the atmosphere, in degrees FAHR. from the absolute zero.				
1	1	26·56929	869·7014	4	1·616735	7·71286	579·801	18	12	11·56929	2·425102	736·5	4·664797	0·03945268	118·2378	118·2377
2	1	70·60445	2311·119	8	10·81664	18·53482	770·3732	36	12	55·60446	32·44992	1473·0	58·70292	0·2096809	279·9632	279·9630
3	2	70·60445	1155·559	8	10·81664	18·53482	770·3732	18	6	27·80223	16·22496	736·5	14·67573	0·05242022	279·9632	279·9628
4	2	187·6224	3070·753	10	27·18387	28·7704	1023·584	36	6	86·3112	81·55161	1473·0	111·90854	0·2786002	401·6815	401·6817
5	4	187·6224	1535·3765	10	27·18387	28·7704	1023·584	18	3	43·1556	40·7758	736·5	27·97713	0·06965	401·6815	401·6815
6	8	498·5821	2040·032	11	51·00245	40·29851	1360·021	18	1·5	60·44776	76·50367	736·5	45·65047	0·092543	493·2893	493·2897
7	20	1324·918	2168·449	11·5	83·68577	54·57992	1807·041	14·4	0·6	65·4959	100·4229	589·2	27·65313	0·04918419	562·2364	562·2361
8	100	12815·505	4194·953	11·9	209·018	106·6709	3495·794	14·4	0·12	128·0051	250·8216	589·2	63·1378	0·0951489	663·5685	663·5692
9	3	105·92437	1155·7525	9	16·75265	22·73109	866·8144	16	4	30·30812	22·33687	654·666	13·16125	0·03932171	334·7069	334·7069
10	2·5	105·92437	1386·903	9	16·75265	22·73109	866·8144	19·2	4·8	36·36974	26·80424	785·6	23·69024	0·0707791	334·7068	334·7068
11	4·5	187·6224	1364·779	10	27·18387	28·7704	1023·584	16	2·$\frac{1}{3}$	38·36053	36·24516	654·666	18·65142	0·0464334	401·6814	401·6816
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17

Additional Note on the preceding Paper. By WILLIAM THOMSON, *M.A., F.R.S., F.R.S.E., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.*

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1. *Synthetical Investigation of the Duty of a Perfect Thermo-Dynamic Engine founded on the Expansions and Condensations of a Fluid, for which the gaseous laws hold and the ratio* (k) *of the specific heat under constant pressure to the specific heat in constant volume is constant; and modification of the result by the assumption of* MAYER'S *hypothesis.*

Let the source from which the heat is supplied be at the temperature S, and let T denote the temperature of the coldest body that can be obtained as a refrigerator. A cycle of the following four operations, *being reversible in every respect*, gives, according to CARNOT'S principle, first demonstrated for the Dynamical Theory by CLAUSIUS, the greatest possible statical mechanical effect that can be obtained in these circumstances from a quantity of heat supplied from the source.

(1.) Let a quantity of air contained in a cylinder and piston, at the temperature S, be allowed to expand to any extent, and let heat be supplied to it to keep its temperature constantly S.

(2.) Let the air expand farther, without being allowed to take heat from or to part with heat to surrounding matter, until its temperature sinks to T.

(3.) Let the air be allowed to part with heat so as to keep its temperature constantly T, while it is compressed to such an extent that at the end of the fourth operation the temperature may be S.

(4.) Let the air be farther compressed, and prevented from either gaining or parting with heat, till the piston reaches its primitive position.

The amount of mechanical effect gained on the whole of this cycle of operations will be the excess of the mechanical effect obtained by the first and second above the work spent in the third and fourth. Now if P and V denote the primitive pressure and volume of the air, and if P_1 and V_1, P_2 and V_2, P_3 and V_3, P_4 and V_4 denote the pressure and volume respectively, at the ends of the four successive operations, we have by the gaseous laws, and by POISSON'S formula and a conclusion from it quoted above, the following expressions:—

Mechanical effect obtained by the first operation $= PV \log \frac{V_1}{V}$.

Mechanical effect obtained by the second operation $= P_2V_2 \cdot \frac{1}{k-1} \cdot \left\{ \left(\frac{V_2}{V_1} \right)^{k-1} - 1 \right\}$.

Work spent in the third operation . . . . . $= P_3V_3 \log \frac{V_2}{V_3}$.

Work spent in the fourth operation . . . . . $=P_3V_3 \cdot \frac{1}{k-1}\left\{\left(\frac{V_3}{V_4}\right)^{k-1}-1\right\}$.

Now, according to the gaseous laws, we have

$$P_1V_1=PV; \quad P_2V_2=P_1V_1\frac{1+ET}{1+ES};$$

$$P_3V_3=P_2V_2; \quad \text{and (since } V_4=V\text{), } P_4=P.$$

Also by POISSON's formula,

$$\left(\frac{V_2}{V_1}\right)^{k-1}=\left(\frac{V_3}{V}\right)^{k-1}=\frac{1+ES}{1+ET}.$$

By means of these we perceive that the work spent in the fourth operation is equal to the mechanical effect gained in the second; and we find, for the whole gain of mechanical effect (denoted by M), the expressions

$$M=(PV-P_3V_3)\log\frac{V_1}{V}=PV\log\frac{V_1}{V}\cdot\frac{E(S-T)}{1+ES}.$$

All the preceding formulæ are founded on the assumption of the gaseous laws and the constancy of the ratio (k) of the specific heat under constant pressure to the specific heat in constant volume, for the air contained in the cylinder and piston, and involve no other hypothesis*. If now we add the assumption of MAYER's hypothesis, which for the actual circumstance is $PV\log\frac{V_1}{V}=JH$, where H denotes the heat abstracted by the air from the surrounding matter in the first operation, and J the mechanical equivalent of a thermal unit, we have

$$M=JH\cdot\frac{E(S-T)}{1+ES}.$$

The investigation of this formula given in my paper on the Dynamical Theory of Heat, shows that it would be true for every perfect thermo-dynamic engine, if MAYER's hypothesis were true for a fluid subject to the gaseous laws of pressure and density, whether, for such a fluid (did it exist), k were constant or not.

It was first obtained by using, in the formula

$$M=JH\varepsilon^{-\frac{1}{J}\int_T^S \mu dt},$$

* From the sole hypothesis that k is constant for a single fluid fulfilling the gaseous laws, and having E for its coefficient of expansion, I find it follows, as a necessary consequence, that CARNOT's function would have the form $\frac{JE}{1+Et+C}$; where C denotes an unknown absolute constant, and t the temperature measured by a thermometer founded on the equable expansions of that gas. From this it follows, that for such a gas subjected to the four operations described in the text, we must have $PV\log\frac{V_1}{V}=JH\frac{1+ES}{1+ES+C}$, and consequently, $M=JH\frac{E(S-T)}{1+ES+C}$, which is Mr. RANKINE's general formula.

which involves no hypothesis, the expression

$$\mu=\frac{J}{\frac{1}{E}+t}$$

for CARNOT's function, which Mr. JOULE had suggested to me in a letter dated December 9, 1848, as the expression of MAYER's hypothesis, in terms of the notation of my "Account of CARNOT's Theory.*" Mr. RANKINE† has arrived at a formula agreeing with it (with the exception of a constant term in the denominator, which, as its value is unknown, but probably small, he neglects in the actual use of the formula), as a consequence of the fundamental principles assumed in his Theory of Molecular Vortices, when applied to any fluid whatever, experiencing a cycle of four operations satisfying CARNOT's criterion of reversibility (being in fact precisely analogous to those described above, and originally invented by CARNOT); and he thus establishes CARNOT's law as a consequence of the equations of the mutual conversion of heat and expansive power, which had been given in the first section of his paper on the Mechanical Action of Heat‡.

2. *Note on the Specific Heats of Air.*

Let N be the specific heat of unity of weight of a fluid at the temperature t, kept within constant volume, v; and let kN be the specific heat of the same fluid mass, under constant pressure, p. Without any other assumption than that of CARNOT's principle, the following equation is demonstrated in my paper§ on the Dynamical Theory of Heat, § 48,

$$kN-N=\frac{\left(\frac{dp}{dt}\right)^2}{\mu\times-\frac{dp}{dv}},$$

where μ denotes the value of CARNOT's function, for the temperature t, and the differentiations indicated are with reference to v and t considered as independent variables, of which p is a function. If the fluid be subject to BOYLE's and MARIOTTE's law of compression, we have

$$\frac{dp}{dv}=-\frac{p}{v};$$

and if it be subject also to GAY-LUSSAC's law of expansion,

$$\frac{dp}{dt}=\frac{Ep}{1+Et}.$$

* Royal Society of Edinburgh, January 2, 1849, Transactions, vol. xvi. part 5.

† On the Economy of Heat in Expansive Engines. Royal Society of Edinburgh, April 21, 1851, Transactions, vol. xx. part 2.

‡ Royal Society of Edinburgh, February 4, 1850, Transactions, vol. xx. part 1.

§ Royal Society of Edinburgh, March 17, 1851, Transactions, vol. xx. part 2.

Hence, for such a fluid,

$$kN-N=\frac{E^2pv}{\mu(1+Et)^2}\text{*}.$$

In the case of dry air these laws are fulfilled to a very high degree of approximation, and, for it, according to Regnault's observations,

$$\frac{pv}{1+Et}=26215, \quad E=\cdot00366$$

(a British foot being the unit of length, and the weight of a British pound at Paris, the unit of force).

We have consequently, for dry air,

$$kN-N=\frac{26215E^2}{\mu(1+Et)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now it is demonstrated, without any other assumption than that of Carnot's principle, in my "Account of Carnot's Theory" (Appendix III.), that

$$\frac{E}{\mu(1+Et)}=\frac{H}{W},$$

if W denote the quantity of work that must be spent in compressing a fluid subject to the gaseous laws, to produce H units of heat when its temperature is kept at t. Hence

$$kN-N=26215E\times\frac{H}{W}=95\cdot947\times\frac{H}{W} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If we adopt the values of μ shown in Table I. of the "Account of Carnot's Theory," depending on no uncertain data except the densities of saturated steam at different temperatures, which, for want of accurate experimental data, were derived from the value $\frac{1}{1693\cdot5}$ for the density of saturated vapour at 100°, by the assumption of the "gaseous laws" of variation with temperature and pressure; we find 1357 and 1369 for the values of $\frac{E}{\mu(1+Et)}$ at the temperatures 0 and 10° respectively; and hence, for these temperatures,

$$\left.\begin{aligned}(t=0)\quad kN-N&=\frac{95\cdot947}{1357}=\cdot07071\\(t=10°)\quad kN-N&=\frac{95\cdot947}{1369}=\cdot07008\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a).$$

Or, if we adopt Mayer's hypothesis, according to which $\frac{W}{H}$ is equal to the mechanical equivalent of the thermal unit†, we have $\frac{W}{H}=1390$; and hence, for all temperatures,

$$kN-N=\frac{95\cdot947}{1390}=\cdot06903 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a').$$

* This equation expresses a proposition first demonstrated by Carnot. See "Account of Carnot's Theory," Appendix III. (Transactions Royal Society of Edinburgh, vol. xvi. part 5.)

† The number 1390, derived from Mr. Joule's experiments on the friction of fluids, cannot differ by $\frac{1}{100}$, and probably does not differ by $\frac{1}{300}$, of its own value, from the true value of the mechanical equivalent of the thermal unit.

The very accurate observations which have been made on the velocity of sound in air, taken in connection with the results of REGNAULT's observations on its density, &c., lead to the value 1·410 for k, which is probably true in three if not in four of its figures. Now, k being known, the preceding equations enable us to determine the absolute values of the two specific heats (kN, and N) according to the hypotheses used in (a) and in (a') respectively; and we thus find,

	Specific heat of air under constant pressure (kN).	Specific heat of air in constant volume (N).	
for $t=$ 0, ...	·2431	·1724,	according to the tabulated values of CARNOT's function.
for $t=$10, ...	·2410	·1709,	
Or, for all temperatures,	·2374	·1684,	according to MAYER's hypothesis.

By the adoption of hypotheses involving that of MAYER, and taking 1389·6 and 1·4 as the values of J and k, respectively, Mr. RANKINE finds ·2404 and ·1717 as the values of the two specific heats.

Hence it is probable that the values of the specific heat of air under constant pressure, found by SUERMANN (·3046), and by DE LA ROCHE and BERARD (·2669), are both considerably too great; and the true value, to two significant figures, is probably ·24.

Glasgow College,
February 19, 1852.

VI. *On a General Law of Density in Saturated Vapours.* By J. J. Waterston, *Esq.* *Communicated by Lieut.-Col.* Sabine, *V.P. and Treas.*

Received June 19, 1851,—Read June 19, 1851.

THE relation between the pressure and temperature of vapours in contact with their generating liquids has been expressed by a variety of empirical formulæ, which, although convenient for practical purposes, do not claim to represent any general law. Some years ago, while examining a mathematical theory of gases, I endeavoured to find out from the French Academy's experiments, if the density of steam in contact with water followed any distinct law with reference to the temperature measured from the zero of gaseous tension. [By Rudberg's experiments, confirmed by Magnus and Regnault, this zero is $-461°$ in Fahr. scale, or $-273°{\cdot}89$ in the Centigrade scale. *Temperatures reckoned from this zero I shall call* G *temperatures to save circumlocution.*] If t represents the G temperature, Δ the density of a gas or a vapour, and p its elastic force, the equation

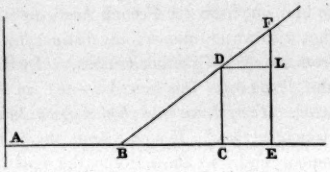
$$t\Delta = p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

represents the well-known laws of Marriotte and of Dalton and Gay-Lussac. The function that expresses a general relation between p and t in vapours must include a more simple function, expressing a general relation between Δ and t. The proper course, therefore, seemed to be to tabulate the quotients $\frac{p}{t}$ from the experiments of the Academy and to project them into a curve. Now, for reasons connected with the *vis viva* theory of gases, which represents the G temperature as a square quantity, I projected these densities as ordinates to *the square root of the* G *temperatures as abscissæ*, and the curve traced out was of the parabolic kind, but of high power. To reduce this, because density is a cubic quantity, I tabulated their cube roots and set them off as ordinates to the same abscissæ. The result was gratifying, for the familiar conic parabola made its appearance. To ascertain whether this curve was exactly the conic parabola, I tabulated the square root of these ordinates, corresponding with the sixth root of the densities, and laid them off as new ordinates to the same abscissæ. The result is shown in the accompanying Chart, Plate VII., under the title *French Academy's Steam.* The observations are denoted by dots thus •, and it will be remarked that they range with great precision in a straight line, any slight divergence being sometimes to the right and sometimes to the left; precisely as might be expected from small errors of observation. Other series of experiments on steam were projected in a similar manner; and it was found that although no two exactly agreed with each other, yet that each set ranged in a straight line nearly.

The vapours of ether, alcohol, and sulphuret of carbon were tried in the same way, and found to conform to the same law. I have since added to the Chart M. Avogadro's observations on the vapour of mercury, which will be found remarkably in accordance; also Dr. Faraday's experiments on liquefied gases, given in the Philosophical Transactions for 1845. Of these, olefiant gas (No. 1, p. 160) is remarkably in accordance; also the nitrous oxide (No. 2, p. 168), ammonia, cyanogen, sulphurous acid, and carbonic acid at the upper part of its range. Muriatic acid, sulphuretted and arseniuretted hydrogen do not show the same regularity.

The coordinates of the points being the square root of the G temperature and the sixth root of the densities, the equation to the straight line that passes through the points expresses the sixth root of the density in terms of the square root of the G temperature.

Thus let t_1= the G temperature corresponding to Δ_1 density of vapour: set off $AC=\sqrt{t_1}$ and $CD=\sqrt[6]{\Delta_1}$. A second observation treated in the same way gives AE, EF, which determines the position of the line of vapour BF. Thus let $AC=g$, and $\cot FBE=h=\frac{DL}{FL}=\left\{\frac{\sqrt{t_2}-\sqrt{t_1}}{\sqrt[6]{\Delta_2}-\sqrt[6]{\Delta_1}}\right\}$ and $g=AB=\sqrt{t_1}-h\sqrt[6]{\Delta_1}$.



The constants g and h are thus determined from two observations, and the equation for the density Δ at any other G temperature is

$$\left\{\frac{\sqrt{t}-g}{h}\right\}^6=\Delta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

and for the pressure,

$$\left\{\frac{\sqrt{t}-g}{h}\right\}^6 t=p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

The following are the equations for the series of observations given on the Chart; the G temperatures are in degrees of Fahr. scale, and the values given to h are made to give the pressure in inches of mercury.

Mercury (Avogadro) . . . . . $p=t\left\{\frac{\sqrt{t}-22{\cdot}606}{20{\cdot}00}\right\}^6$.

Oil of turpentine (Ure).

Salt water saturated (Watt).

Water (French Academy and Southern) . $p=t\left\{\frac{\sqrt{t}-19{\cdot}492}{10{\cdot}830}\right\}^6$.

Alcohol, sp. gr. ·813 (Ure) . . . . . $p=t\left\{\frac{\sqrt{t}-19{\cdot}287}{9{\cdot}800}\right\}^6$.

Sulphuret of carbon (Marx) . . . . . $p=t\left\{\frac{\sqrt{t}-16{\cdot}254}{12{\cdot}76}\right\}^6$.

Sulphuric ether (Dalton) . . . . . $p=t\left\{\frac{\sqrt{t}-16{\cdot}860}{10{\cdot}990}\right\}^6$.

Sulphurous acid (FARADAY) . . . . . $p=t\left\{\frac{\sqrt{t}-14{\cdot}667}{11{\cdot}194}\right\}^6$.

Cyanogen (FARADAY) . . . . . $p=t\left\{\frac{\sqrt{t}-13{\cdot}846}{11{\cdot}542}\right\}^6$.

Ammonia (FARADAY) . . . . . $p=t\left\{\frac{\sqrt{t}-13{\cdot}317}{11{\cdot}050}\right\}^6$.

Arseniuretted hydrogen (FARADAY) . . . $p=t\left\{\frac{\sqrt{t}-12{\cdot}929}{10{\cdot}264}\right\}^6$ (?).

Sulphuretted hydrogen (FARADAY) . . . $p=t\left\{\frac{\sqrt{t}-12{\cdot}957}{9{\cdot}878}\right\}^6$ (?).

Muriatic acid (FARADAY) . . . . . $p=t\left\{\frac{\sqrt{t}-12{\cdot}060}{9{\cdot}413}\right\}^6$ (?).

Carbonic acid (FARADAY) . . . . . $p=t\left\{\frac{\sqrt{t}-11{\cdot}997}{8{\cdot}857}\right\}^6$.

Nitrous oxide (FARADAY), No. 2, p. 168 . $p=t\left\{\frac{\sqrt{t}-8{\cdot}936}{11{\cdot}604}\right\}^6$.

Olefiant gas (FARADAY), No. 1, p. 160 . . $p=t\left\{\frac{\sqrt{t}-10{\cdot}352}{10{\cdot}152}\right\}^6$.

While projecting different sets of observations of the same vapour, the attention is forcibly drawn to the tendency which the line shows to alter its inclination on the most trifling change of circumstances. This inclination depends on the value of the denominator h. It will be remarked that this constant is nearly the same for the vapours of ether and of water, also for a considerable number of the liquified gases. In fact the difference is not greater than might be caused by a slight want of purity. Even the specific nature of the vessel in which the observations are made seems materially to affect the results. Dr. FARADAY employed tubes of from one-fifth to one-seventh inch bore. I believe it will be found that the density is considerably less in these tubes than in larger vessels; I have had proof of this in the case of ether heated in such tubes to 280°. The capillary attraction of the sides even at high pressures seems to affect the whole surface of the liquid, and to check the formation of vapour. We must not forget also, that Dr. FARADAY's temperatures are confessedly not so low as a correct thermometer would have indicated. The liquid he employed to measure the temperature was alcohol, and the degrees were graduated with the same capacity as between 32° and 212°. Now from the experiments that have been made on the expansion of alcohol by M. GAY-LUSSAC, and more recently by M. MUNCKE, it appears that the mean contraction between 32° and 212° is 6·2 thousandths of the volume at 173°, its boiling-point, and that this amount diminishes very sensibly with the temperature; and if it continued to do so at the same rate from 32° to −100°, we should have Dr. FARADAY's −100° equal to less than −200° by the air-thermometer. It is clear that until the value of these degrees are known with reference to the air-thermometer, the importance of Dr. FARADAY's observations cannot be fully estimated.

It will be observed from the Chart, that the observations on the vapour of water below 80° show a small excess of density above what is required by the line drawn through those at higher temperatures, and the contour of their projection is a curve convex to the axis, and very nearly the same as would be caused by the pressure of a very minute quantity of air; but the character of the late experiments of M. Regnault and Magnus forbid this mode of explaining the slight divergence. It is probable that it is connected with the change of condition that takes place at 32°, and the effects of which may be sensible up to 80° or 90°.

It is a curious circumstance that the law of expansibility of water also becomes disturbed at about the same temperature. As this has not been remarked, I have given the proof of it by projecting M. Despretz's observations* on the Chart. If the volume is made ordinate to the square root of the G temperature as abscissa, these observations above 25° Centigrade or 77° Fahr., trace out *in the most exact manner a conic parabola*. The equation is

$$a(v-\theta)=(\sqrt{t}-\varphi)^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

in which v is the volume of the G temperature t in terms of its volume unity at 39°·2 Fahr., or 4° Centigrade (its point of maximum density), $a=352{\cdot}38$, $\theta={\cdot}99872$, and $\varphi=21{\cdot}977$ or $\varphi^2=483°$.

In the Chart I have made $\sqrt{v-\theta}$ the ordinate, and $\sqrt{t}$ the abscissa. The proof of the position is, that in this mode of projection the observations range themselves exactly in a straight line from about 77° upwards†, and that below this temperature there is both less decrement of volume and less decrement of density than is required by the law that is followed above that temperature. Other liquids appear to follow the same rule of expansion, but the range of the observations is too small as yet to found upon them any general conclusion.

If these equations for the expansion of water and the density of its vapour hold good at high temperatures, they would have a common density at 1108° Fahr., its amount being $\frac{1}{1{\cdot}878}$.

The equation for the density of steam, at any G temperature under this, in terms of the density of water unity at 39°·2, is

$$\Delta=\left\{\frac{\sqrt{t}-19{\cdot}492}{22{\cdot}2745}\right\}^6. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

M. Cagniard de la Tour observed the sudden conversion of water into steam at a much lower temperature, but his observations are at variance with the laws of Marriotte and Dalton, and Gay-Lussac; and although a slight divergence from these laws has been discovered by M. Regnault, it is quite in the opposite direction to that which M. Cagniard de la Tour's observations require.

* Ann. de Chem. vol. lxx.

† If these ordinates are projected to ordinates $=t$ instead of $\sqrt{t}$, the line is a distinct and regular, though flat, curve convex to axis. This, if confirmed at high temperatures, will prove that the density of liquids as well as vapours has reference to the zero of gaseous tension.

VII. *On the Electro-Chemical Polarity of Gases.*

By W. R. Grove, *Esq.*, *M.A.*, *F.R.S.*

Received January 7,—Read April 1, 1852.

THE different effect of electricity upon gases and liquids has long been a subject of interest to physical inquirers. There are, as far as I am aware, no experiments which show any analogy in the electrization of gases to those effects now commonly comprehended under the term electrolysis. Whether gases at all conduct electricity, properly speaking, or whether its transmission is not always by the disruptive discharge, the discharge by convection, or something closely analogous, is perhaps a doubtful question; but I feel strongly convinced that gases do not conduct in any similar manner to metals or electrolytes.

In a paper published in the year 1849*, I have shown that hydrogen or atmospheric air intensely heated, showed no sign of conduction for voltaic electricity even when a battery of very high intensity was employed.

In the Eleventh, Twelfth and Thirteenth Series of Faraday's Experimental Researches, the line of demarcation between induction across a dielectric and electrolytic discharge is repeatedly adverted to; induction is regarded as an action of contiguous particles, and as a state of polarization anterior to discharge, whether disruptive, as in the case of dielectrics, or electrolytic, as in electrolytes. See §§ 1164—1298—1345—1368, &c.

Mr. Gassiot, in a paper published in the year 1844†, has shown that the static effects, or effects of tension, produced by a voltaic battery, are in some direct ratio with the chemical energies of the substances of which the battery is composed; in other words, that in a voltaic series, whatever increases the decomposing power of the battery when the terminals are united by an electrolyte, also increases the effects of tension produced by it, when its terminals are separated by a dielectric.

In none of the above papers, and in no researches on electricity of which I am aware, is there any experimental evidence that the polarization of the dielectric is or may be chemical in its nature, that, assuming a dielectric to consist of two substances having antagonist chemical relations, as for instance, oxygen and hydrogen, the particles of the oxygen would be determined in one direction, and those of the hydrogen in the other; the only experimental result bearing on this point with which I am acquainted, is the curious fact which was observed by Mr. Gassiot and

* Philosophical Transactions, 1849, p. 55.

† Philosophical Transactions, 1844, p. 39.

some other electricians who experimented with him in the year 1838, viz. that when two wires forming the terminals of a powerful battery were placed across each other, and the voltaic arc taken between them, the extremity of the wire proceeding from the positive end of the battery was rendered incandescent, while the negative wire remained comparatively cool; it was at that time believed that there was some effect exhibited here *extra* the voltaic circuit. Shortly afterwards I showed that with all, or at all events a great number of metals, the positive terminal was more heated than the negative, and that the portion of the crossed wire which was positive became more incandescent than that of the negative, from the greater heating effect developed at the point when the disruptive discharge took place. I suggested as an explanation of this phenomenon, the possibility that in air, as in water, or other electrolyte, the oxygen or electro-negative element was determined to the positive terminal, and that from the union of the metal with that oxygen a greater heating effect was developed. This, with some other impressions, I mentioned in a letter to my friend Dr. Schönbein, not intended for publication, but which shortly afterwards found its way into print*.

Though by no means thinking that this explanation was in every respect satisfactory, there were many arguments in its favour, and the fact strongly impressed my mind as evincing a very striking difference in character between the effect of the discharge at the positive and negative terminals, and as presenting, as far as it went, a distant analogy to the effect of electrolysis.

In the year 1848, while experimenting with Mr. Gassiot with a nitric acid battery consisting of 500 well insulated cells, I made the following experiment:—Two wires of platinum $\frac{1}{40}$th of an inch in diameter, forming the terminals of the battery, were immersed in distilled water; the negative wire was then gradually withdrawn until it reached a point a quarter of an inch distant from the surface of the water. A cone of blue flame was now perceptible, the water forming its base, and the point of the wire its apex; the wire rapidly fused, and became so brilliant that the cone of flame could be no longer perceived, and the globule of fused platinum was apparently suspended in air and hanging from the wire; it appeared sustained by a repulsive action like a cork ball on a *jet d'eau*, and threw out scintillations in a direction away from the water. The surface of the water at the base of the cone was depressed, and divided into little concave cups, which were in a continual agitation. When the conditions were reversed and the negative wire immersed, the positive wire being at the surface, similar phenomena ensued, but not nearly in so marked a manner; the cone was smaller, and its base much more narrow in proportion to its height.

This experiment, the beautiful effect of which requires to be seen to be appreciated, indicates a new mode of transmission of electricity partaking of the electrolytic and disruptive discharges. Not possessing a battery of this enormous intensity, I have not been able to examine this phenomenon more in detail; but I have from time to time

* Philosophical Magazine, 1840, p. 478.

made many other experiments on the voltaic arc taken in various gaseous media, with the view of ascertaining the state of the intervening media anterior to, during, and after the discharge; these experiments have hitherto given me no results of any value. In the voltaic arc, the intense heat developed so affects the terminals and so masks the proper electrical effect, that the difficulty of isolating the latter is extreme; and I have latterly sought for some modified form of electric discharge which should be intermediate between the voltaic arc and the ordinary Franklinic discharge, or that from the prime conductor of a frictional machine; for something, in short, which should yield greater quantitative effects than the electrical machine, but not dissipate the terminals, as is done by the voltaic arc.

An apparatus, to which M. Despretz was kind enough to call my attention recently at Paris, seemed to promise me some aid in this respect. It was constructed by M. Ruhmkorff, on the ordinary plan for producing an induced current, viz. a coil of stout wire round a soft iron core, with a secondary coil of fine wire exterior to it, having an ingenious self-working contact breaker attached; from the attention paid to insulation in the construction of this apparatus, very exalted effects of induction could be procured. Thus in air rarefied by the air-pump, an aurora or discharge of 5 or 6 inches long could be obtained from the secondary coil, and in air of ordinary density a spark of one-eighth of an inch long.

I procured one of these apparatus from M. Ruhmkorff; the size of the coil portion of the apparatus is 6·5 inches long, 4 inches diameter; the length of the wires forming the coils are (I give M. Ruhmkorff's measurements) stout wire, 30 metres long, 2 millimetres diameter, 200 convolutions; fine wire, 2500 metres long, $\frac{1}{4}$ metre diameter, 10,000 convolutions. These measurements will only be taken as approximative, and indeed the exact size is immaterial to the consideration of the experiments which I am about to detail. I will not give my experiments in the order in which I made them, as I should have to describe many fruitless ones, but I will place first that which I consider the most important and fundamental.

1st. On the plate of a good air-pump was placed a silvered copper plate, such as is ordinarily used for Daguerreotypes, the polished silver surface being uppermost. A receiver, with a rod passing through a collar of leathers, was used, and to the lower extremity of this rod was affixed a steel needle, which could thus be brought to any required distance from the silver surface; a vessel containing potassa fusa was suspended in the receiver, and a bladder of hydrogen gas was attached to a stop-cock, another orifice enabling me to pass atmospheric air into the receiver in such quantities as might be required*. A vacuum being made, hydrogen gas and air were allowed to enter the receiver in very small quantities, so as to form an attenuated atmosphere of the mixed gas: there was no barometer attached to my air-pump, but from separate experiments I found the most efficient extent of rarefaction for my purpose was that indicated by a barometric height of from half to three-quarters of

* See a figure and description of the apparatus at the end of this paper.

an inch of mercury; and except where otherwise stated, a similarly attenuated medium was employed for all the following experiments.

Two small cells of the nitric acid battery, each plate exposing 4 square inches of surface, were used to excite the coil machine, and the discharge from the secondary coil was taken between the steel point and the silver plate. The distance between these was generally $=0{\cdot}1$ of an inch, but this may be considerably varied. When the plate formed the positive terminal, a dark circular stain of oxide rapidly formed on the silver, presenting in succession yellow, orange and blue tints, very similar to the successive tints given by iodizing in the ordinary manner a Daguerreotype plate. Upon the poles being reversed and the plate made negative, this spot was entirely removed, and the plate became perfectly clean, leaving, however, a dark, polished spot occasioned by molecular disintegration, and therefore distinguishable from the remainder of the plate.

The experiment was repeated a great many times, and with varying proportions of gas, and I found that with proportions varying from equal volumes of hydrogen and air to those of one volume of the former to two and a half of the latter, the experiments succeeded; better, I should say, when there was rather an excess of hydrogen as compared with the equivalent of oxygen in the atmospheric air; about one volume of hydrogen to one and a half of air succeeded well; when excess of air was present, oxidation took place whether the plate was positive or negative, and when excess of hydrogen was present no oxidation took place.

2nd. I experimented with an air vacuum (to borrow an expression of Dr. Faraday), and found that oxidation took place whether the plates were positive or negative, but in different degrees; when the plate was positive, a small circular spot was rapidly formed, quickly deepening in colour, and apparently eating into the plate; when the plate was negative, a large diffuse spot was formed, the oxidation was more slow, and the plate not so rapidly corroded.

3rd. I now operated with a hydrogen vacuum; when the plate was clean no discoloration took place, the plate retained its polish, though after a long continuance of the discharge a molecular change was perceptible, producing a frosted appearance similar to the mercurialized portions of a Daguerreotype.

When the plate had been previously oxidated by the discharge in an air vacuum, the oxidation was rapidly and beautifully cleared off by the discharge in the hydrogen vacuum, and this whether the plate was positive or negative, the effect being, however, better and more rapidly produced in the latter case.

4th. I substituted respectively for the steel needle, wires of copper, silver and platinum, and found the effect produced by all and with nearly equal facility; if there were any difference, the platinum point was the least efficient; this may be due to the peculiar effect of platinum in itself combining the gases, or to its inoxidable character, the oxygen being thrown off from its surface, and not uniting with it as with the more oxidable metals; the flame or luminous appearance which

surrounded the wire when the platinum was negative, was larger and more diffuse than with the other metals.

5th. As air, notwithstanding its containing a great excess of nitrogen, gave an effect of oxidation at both electrodes, though different in degree, I increased the proportion of nitrogen by passing into the receiver nitrogen which had been formed by the slow combustion of phosphorus, the phosphorous acid having been well washed away, and potash being always in the receiver; no more air was allowed to be present than the very small quantity contained in the apertures of the stopcock; with this mixture, viz. a maximum of nitrogen and a minimum of oxygen, and rarefied as before, a similar effect was produced to that shown in the mixture of air and hydrogen, the positive plate being oxidated by the discharge, and the spot when made negative being reduced. The effect of reduction was not so rapid or so readily produced as when hydrogen was used, but was very decided.

6th. With nitrogen, as much deprived of oxygen as I could procure, the colours of oxidation were not exhibited, but a dark spot apparently due to disintegration was produced, which was not removed by the plate being made negative; if, however, the coloured spot was produced by the plate being made positive in an air vacuum, they were removed by the plate being made negative in a nitrogen vacuum, leaving, however, a darker spot than that which was exhibited when they were reduced in hydrogen. Even when produced in an air vacuum, and then a very perfect exhaustion effected, such as would reduce the mercury in the barometer to the height of $\frac{1}{30}$th of an inch, the spot was partially reduced when the plate was made negative.

7th. An oxyhydrogen vacuum was formed, the gases being in the proportion in which they form water; and thanks to the attenuated atmosphere, it was easy to take the discharge in this mixture without producing detonation or any sudden combination of the gases, a possibility pointed out by Grotthus*. With this mixture the effect took place as with the mixture of atmospheric air and hydrogen. I expected it to have been more efficient, but it was rather less so than the mixture of air and hydrogen; whether it be that the presence of nitrogen lessens the tendency to combine of the gases oxygen and hydrogen, and thus enables the electrical polarization and discharge to operate more efficiently, whether the nitrogen has a specific effect in aiding the electro-chemical effect, as I have shown it has in one peculiar case†, or whether any unknown effect of nitrogen is concerned, I do not undertake to pronounce; I can only say that in several repetitions of the experiment, it appeared to me that the mixture of atmospheric air and hydrogen was more efficient in exhibiting this phenomenon than that of oxygen and hydrogen.

8th. Different proportions of oxygen and hydrogen were employed, and here also I found that within a tolerably wide margin I could vary the proportion of the gases; three volumes of hydrogen to one volume of oxygen I found to be a very efficient mixture.

* Annales de Chimie, vol. lxxxii.

† Philosophical Transactions, 1843, pp. 110, 111.

9th. I now substituted for the silver plate, plates of the following metals:—bismuth, lead, tin, zinc, copper, iron and platinum, the former three metals being burnished, the latter polished.

Bismuth showed the effect nearly, if not quite as well as silver; it was oxidated in an air vacuum, reduced in a hydrogen vacuum, and oxidated or reduced in the mixed gas according as it formed the positive or negative terminal.

Lead oxidated easily, but the spot of oxide could with difficulty be reduced. Tin, zinc and copper required the admission of a great quantity of air to produce oxidation; and I could not succeed in reducing the oxide by the electrical discharge, at least so as to restore the polish of the plate; a blackening effect was in some degree produced. Iron was not oxidated until the receiver was nearly filled with air, and then a small spot of rust was formed which I could not reduce. With all the metals a slight whitish film like the mercurialized portion of a Daguerreotype was visible beyond the circle marked by the discharge when the plate was rendered positive, which film was removed by negative electrolization in a hydrogen vacuum; it seemed to me that this film, as well as others among those I have described, was affected by light, but I did not turn aside to examine this effect. Platinum showed no effect either of oxidation or reduction.

10th. As it was impossible to operate with an atmosphere of chlorine with the apparatus which I possessed, and wishing to vary the electro-negative element, I iodized a silver plate by the vapour of iodine to a deep blue colour, and then made it negative in an atmosphere of hydrogen; the iodine was beautifully removed in a circle or disc opposite the point which formed the positive terminal.

11th. I now substituted for the coil apparatus a very good electrical machine, the cylinder of which was 16 inches diameter, and the prime conductor of which, when the machine was properly excited, gave a spark of 8 inches long. With this machine, and in an attenuated atmosphere of one volume hydrogen plus two of atmospheric air, I produced the effects of oxidation and reduction very distinctly, the plate being in turn connected with the conductor and with the ground; but the comparative minuteness of the spot after many turns of the machine, showed the great superiority of the coil machine for producing quantitative effects over the ordinary electrical machine; and I question whether I should have detected the phenomenon with the latter, had I not become previously well acquainted with it by the former apparatus. Probably an extensive series of the water battery or a steam hydro-electric machine would succeed equally well, or better than the coil machine.

12th. A solution of hyposulphite of soda removed the spots formed by electrization from the silver plate just as it removes the iodine from an iodized plate.

13th. In some of the above experiments I remarked a tendency in the spots produced by the discharge, to show circles or zones of oxidation in different degrees, and in a more marked manner than would be accounted for by the different colours of the thin films of oxide formed. I determined to examine this effect, and selected,

after some experiments, an atmosphere of one volume oxygen mixed with four volumes of hydrogen, and attenuated by the air-pump as in the previous experiments. The plate was made positive, and the point was placed successively opposite different portions of the silver plate, at distances of $\frac{1}{50}$th, $\frac{2}{50}$ths, $\frac{3}{50}$ths, $\frac{4}{50}$ths and $\frac{5}{50}$ths of an inch. The results are given, as nearly as I can copy them, in the accompanying Plate, figs. 1 to 5.

The colour of the central spot was a yellow-green in the centre, surrounded by a blue-green, then a clear ring of polished silver, then an outer ring crimson, with a slightly orange tint on the inner side, and deep purple on the outer; the exterior portion of the spot was, as far as my eye could judge, of a colour complementary to the interior of the external ring, and the central portion of the spot of a colour complementary to the exterior portion of the ring. The colours varied with the time, density of gas and other conditions, but generally showed this complementary tendency. Symptoms of a faint polished ring were visible beyond the outer ring, and could be rendered more distinct by breathing on the plate. As the distance between the point and the plate was increased the colours became fainter, and the rings more diffuse, and beyond the distance I have given nearly lost their defined character; but the first three distances, or those of $\frac{1}{50}$th, $\frac{2}{50}$ths and $\frac{3}{50}$ths of an inch, gave very beautifully defined rings. The luminous appearance on the needle in these experiments extended from three-fourths of an inch to an inch from the point. Frequently a small polished speck was visible, exactly opposite the point of the needle. See fig. 6. When the plate was made negative, the other conditions being the same, a polished space appeared opposite the point of the needle, surrounded by a dusky and ill-defined areola; its colour, when regarded from a point opposite the incident light, was brown tinged with purple; and when in the same direction as the light, a greenish white, similar to the tint seen on mildew or on some of the lichens: these spots were very different from the positive spots, and in some degree the converse of them; but they were not nearly so well defined or capable of being produced with the same uniformity. I have endeavoured to represent one of them at fig. 7.

14th. In order to ascertain whether the polished ring intervening between the oxidated central spot and oxidated external ring were a mere negation of effect or an antithetic polar effect, such as would occasion reduction, I formed in an air vacuum two large spots on a silver plate, with one the plate being made negative, and with the other positive, oxidating them until they began to pass from deep orange to purple. I then perfectly exhausted the receiver, swept it with the gas employed in the last experiment, and then took the discharge in a vacuum of that gas, viz. one volume oxygen+four hydrogen; the plate being positive and the needle $\frac{3}{50}$ths of an inch over the centre of each spot in turn, a ring of clear polish was formed rapidly in both the dark discs, just at the distance where the ring of polish appeared in the last experiment. I then exposed a clean portion of the plate to the needle without

any other change, and on allowing the discharges to pass, formed the rings just as in the last experiment.

15th. I examined some of the spots with an achromatic microscope, magnifying 200 diameters; I could not, however, discover any feature which the naked eye did not show, or any peculiar molecular state; the polishing scratches on the plate were highly magnified, but the electrized spots only showed more dimly the colours or the lights and shadows which they exhibited to the naked eye.

16th. I took the discharge on a silver plate in vacua of the following gases respectively:—Oxygen, protoxide of nitrogen, deutoxide of nitrogen, carbonic acid, carbonic oxide and olefiant gas.

The first four gases presented nothing remarkable, the plate was oxidated whether positive or negative, as in a vacuum of atmospheric air. In the protoxide of nitrogen the colour of the discharge was a beautiful crimson on both terminals.

In deutoxide of nitrogen a greater tendency to reduction was shown when the plate was negative than in the other three gases, and there was also a tendency to the formation of rings. In carbonic oxide the plate was oxidated when positive, and the oxide reduced when negative, just as with a vacuum of air and hydrogen, but rather more slowly; with a mixture of five volumes of carbonic oxide and one volume of oxygen, the rings were formed very distinctly, particularly if the plate was made negative first, and then positive. The luminous spot on the plate, when positive in this gas, was coloured green.

When the plate was negative in olefiant gas it darkened, showing the rings of colour produced by thin plates, and very distinct from the other rings of which I have spoken. After a short time a pulverulent deposit was formed on the plate, giving brilliant sparks or stars of light which were not shown by any other gas.

This deposit was too minute for analysis, but I have no doubt, from the gas used and the appearances presented, it was carbon.

I have given in the above experiments the conditions under which they succeeded best; but upon repetition, although the exact volumes of gases and other conditions were carefully attended to, they sometimes required a slight alteration to succeed, variations taking place from causes which I could not detect; thus it was sometimes necessary to add a little more hydrogen, sometimes a little more oxygen or air, to alter slightly the state of attenuation in the gas, &c.

The necessarily varying condition of the battery, and the state of the contact breaker, slight impurities in the gases or on the surface of the plates would be quite sufficient to account for these irregularities. I mention them for the guidance of any one who may wish to repeat the experiments; a very little practice will enable any electrician to have the results at his command. When there is too great a proportion of air or oxygen, oxidation takes place at both poles; when too much hydrogen, reduction takes place at both; and to effect oxidation or reduction by reversing the

direction of the discharge, an intermediate condition is requisite; so if the gas be not sufficiently attenuated the oxidation is too rapid, and the plate too much corroded to bring out the effects clearly; if too much attenuated, too long a time is required and the effect is feeble and indistinct.

I have above selected all the experiments which I consider material in this, I believe, new class of phenomena. The spots produced by electrical discharges, both on conducting bodies and on electrics, have been before noticed and experimented on, one class by Priestley*, and another class by Karsten† and others, but as far as I am aware no distinct electro-chemical action in dry gases, depending upon the antithetic state of the terminals and presenting a definite relation of the chemical to the electrical actions in gaseous media, has been pointed out. I now proceed to consider the relation which these results bear to other electrical phenomena.

As may be gathered from my opening remarks, the experiments above detailed appear to me to furnish a previously deficient link in the chain of analogy connecting dielectric induction with electrolysis. The only satisfactory rationale which I can present to my own mind of these phenomena is the following. The discharges being interrupted (as is evident from the nature of the apparatus, and may be easily proved by agitating a mirror near them and regarding their reflected images in the moving mirror), the gaseous medium is polarized anterior to each discharge, and polarized not merely physically, as is generally admitted, but chemically, the oxygen or anion being determined to the positive terminal or anode, and the hydrogen or cation being determined to the negative terminal or cathode; at the instant preceding discharge there would then be a molecule or superficial layer of oxygen or of electro-negative molecules in contact with the anode, and a similar layer of hydrogen or of electro-positive molecules in contact with the cathode, in other words, the electrodes in gas would be polarized as the electrodes in liquid are. The discharge now takes place, by which the superficial termini of metal or of oxide, as the case may be, are highly ignited or brought into a state of chemical exaltation at which their affinities can act; the anode thus becomes oxidated, and the cathode, if an oxide, reduced. I have elsewhere‡ shown strong reasons for assuming that the electric or voltaic discharge, the moment polarity is subverted, may be regarded as an intensely heated state of the electrodes, and of the intermedium across which it passes; and my present explanation is perfectly consistent with and derivable from my previous views of the disruptive discharge.

Two other theories might be proposed to account for the phenomena I am considering; the one, that the disruptive discharge itself is analogous to the electrolytic, and that the oxygen and hydrogen are reciprocally transferred by the discharge itself; this would not, I think, be consistent with the generally known facts connected with the discharge, and is entirely ineffectual in explaining the experiments

* History of Electricity, 2nd edition, p. 624.

† Archives de l'Electricité, vol. ii. p. 647; vol. iii. p. 310.

‡ Philosophical Transactions, 1847, pp. 10, 16, 21. Correlation of Physical Forces, p. 50, 2nd edition.

2nd and 3rd, where either the positive or negative terminal can be made either to oxidate or reduce, according to the nature of the chemical medium present, while these experiments are entirely in accordance with, and the results of them flow as a necessary consequence of, the view first advanced. The other theory which may be advanced is, that by dielectric induction the gases may be bodily separated, a layer, not molecular, but corporeal or voluminous, if I may be allowed these expressions, of oxygen being developed on the side next the anode, and one of hydrogen next the cathode, the gas intervening between the terminals being thus divided, as it were, into two halves: this would certainly be a most curious phenomenon, but I believe it to be so inconsistent with the vast mass of accumulated facts in electrical science, and likely to have produced in cosmical phenomena so many results which, if existing, must long ere this have been detected, that I will not do more than advert to it.

I have adopted the views which I have first stated as being the least removed from ordinary theories or modes of regarding electrical phenomena, and because in the present instance I can present the phenomena in no other way which is in the least degree satisfactory to my own mind, while this view to me well accounts for them. Assuming then for the present this view, we get a close approximation, I may say an identity of the state of polarization in gaseous non-conducting dielectrics, and in electrolytes anterior respectively to discharge or to electrolysis.

Faraday observes, Experimental Researches, 1164, "In an electrolyte induction is the first state, and decomposition the second." My present experiments show, I believe, that in induction across gaseous dielectrics there is a commencement, so to speak, of decomposition, a polar arrangement not merely of the molecules, irrespective of their chemical characters, but a chemical alternation of their forces, the electro-negative element being determined or directed, though *not travelling* in one direction, and the electro-positive in the opposite direction.

This arrangement is only evidenced at present, as it is in electrolysis, by the action at the polar extremities or termini of the dielectric; possibly future reasearches may show, by the action of polarized light, by magnetism or some other means of analysis, that the polarity extends, as we theoretically believe it does, through the whole intervening matter.

In the Experiment No. 5 with oxygen and excess of nitrogen, reduction takes place by the effect of negative electricity and heat, at least there seems every reason from analogy to believe that the effect of the nitrogen is only negative, protecting the plate from oxygen, or at furthest catalytic, aiding the reduction as sulphuric acid aids the electrolysis of water. Upon the state of association of the gases in what is generally called mixture, I venture an opinion with the greatest diffidence. I have always inclined to the opinion that the difference between physical admixture, as it is termed, of gases and chemical union, is one of degree, and the views of Dalton ever presented to my mind grave difficulties*. My present results seem to me in favour of

* Philosophical Transactions, 1843, p. 112.

the chemical view, as otherwise we can scarcely imagine electricity as effecting in the instances given a merely physical separation; it may indeed be said that there is composition and decomposition produced by the same discharge, but this is very difficult to conceive, and can hardly apply to the cases of oxygen with nitrogen and of carbonic oxide.

In the experiments I have detailed, the flame or visible effect of the electric discharge coincided with the chemical effect; when the plate was positive, a small globule of flame of a purple colour was visible on the part of the plate attacked, and a bluish flame extended over an inch or more of the needle. When the plate was negative, a wider and less defined disc of blue flame extended over the part of the plate opposed to the positive point, like a splash of liquid thrown upon it, and a pencil of light appeared on the point. Sometimes, but not always, this flame avoided the oxidated portion, probably from its inferior conducting power; and when this was the case reduction took place in a much slighter degree, or not at all; sometimes, and I observed this particularly with bismuth, the flame attached itself to the oxidated portion, and then reduction immediately followed. Here, as in all the electrical phenomena that I can call to mind, we get the visible effects of electricity associated with physical changes in the matter acting, changes of state in the terminals, polarization of the intervening medium, or both*. These experiments furnish additional arguments for the view which I have long advocated, which regards electricity as force or motion, and not as matter or a specific fluid†.

The chemical polarity of gases shown, as I believe, in this paper, associates itself with an experiment which I made known in a lecture at the London Institution in the year 1843‡, and which was subsequently verified by Mr. Gassiot§ with more perfect apparatus than I possessed, viz. that when discs of zinc and copper are closely approximated, but not brought in contact, and then suddenly separated, effects of electrical tension are exhibited, the one disc making the electroscope diverge with positive, and the other with negative electricity, showing that the effects ascribed by Volta to contact can be produced without contact, and by mere approximation, the intermediate dielectric being polarized, or a radiation analogous, if not identical, with that which produces the images of Moser taking place from plate to plate.

The present experiments also associate themselves with the gas battery, where, though an electrolyte is used as the means of making the action continuous, or producing what is called current electricity, the initiating effect is gaseous polarity, the films of gas in contact with the respective plates of platinum having antithetic chemical and electrical states.

* Gases at present believed to be elementary, probably undergo a *quasi* chemical polarization by electricity; thus portions of oxygen are changed to ozone, &c. See a recent paper by MM. Fremy and E. Becquerel, Comptes Rendus, Paris, March 15.—Note added to the Proof, W. R. G.

† Printed Lecture at the London Institution, 1842, p. 28. Correlation of Physical Forces, p. 48.

‡ Literary Gazette, 1843, p. 39.

§ Philosophical Magazine, October, 1844.

The results detailed in Experiment 13th, appear to open a new field of research. PRIESTLEY observed concentric circles produced by the electrical discharge from a powerful Leyden battery, which he describes as consisting of minute cavities and globules of fused metal*. In my experiments there is an alternation of oxidation and reduction, a medium capable of producing both being present; the lateral effect and complementary colours have to my mind something closely resembling the phenomena of interference in light, although from the polar character of the force, it is difficult to imagine any precisely analogous condition of electricity. The discharge taking place from different parts of the needle and extending from its point to a considerable distance over its surface, would give different lengths for the lines of polarization and discharge to the different parts of the disc on the silver plate affected by the discharge; and assuming electricity to be propagated by undulations, there would be interference; but instead of alternations of light and darkness we get alternations of positive and negative electricity. The ring of polished metal between the central spot and the exterior ring, quite distinguishes these rings from the ordinary colours of thin plates, *i. e.* colours, the annular succession of which depends only on the different thicknesses of the film; here doubtless the colours of the oxidated portions are colours of thin plates. Experiment 14 shows clearly that the action by which the polished ring is formed is a polar action of the discharge, and not a mere absence of action.

When the plate is negative, the effect is, as I have observed, less marked and more uncertain; but in this case it should be recollected that the visible discharge issues from the point, and does not extend, or extends to a very small degree, over the surface of the needle.

If the phenomena were such that the central portion were always clear, while around it was one, and one only circle of oxide, it might be accounted for by the hypothesis, that the lines of polarization and discharge between a point and flat surface, assume the form of a hollow cone; but a cone of negative bounded by cones of positive action, still gives the idea of some lateral fits or phases of undulation.

The high rarefaction of the medium by the discharge, and its intermitting character, might occasion pulsations by the inrushing of the surrounding gas, and thus vacua in circles might be formed at the places where the action of oxidation is rendered null; but this view is, I think, inadmissible; it does not account for the effects obtaining only in certain mixtures, it does not account for the reducing action, the plate being positive, and presents other difficulties. The point involved in Experiments 13 and 14, though not perhaps the least valuable one given in this paper, presents apparently a wide field of inquiry; I therefore will not further dilate on it at present, and hope to make it the subject of future investigation.

* History of Electricity, 2nd edition, p. 624.

December 27, 1851.

Postscript, April 24th.

I may, I trust, be permitted to add to this paper one or two experiments on the subject last discussed. Assuming that the alternations of oxidation and reduction were produced by interference in consequence of the discharge proceeding from successive points of the terminal or terminals, a difference of effect might be anticipated if the electricity passed from a point only, and not from a line as was the case in Experiment 13. I therefore sealed a platinum wire $\frac{1}{30}$th of an inch in diameter into a piece of glass tubing, and then ground the extremity to a flat surface, so that the section only of the wire was exposed; this wire was placed opposite, and at 0·07 of an inch distance from the polished silver plate, in a mixture of one volume of oxygen with five volumes of hydrogen attenuated until the barometer stood at half an inch; discharges from the secondary coil were then passed, the plate being positive, and a round dark spot of oxide formed represented at fig. 8; the platinum sealed in glass was then removed and the steel needle substituted for it, all else, viz. plate, gas, barometer height, &c., being the same: the system of rings represented at fig. 9 was now produced.

Another experiment was made, directed to the same point: a wire of copper 0·04 inch diameter, and a thread of glass of the same diameter were attached by sealing-wax at their extremities in a horizontal position 0·025 of an inch from different parts of a silver plate, being insulated from the silver by the wax interposed at the extremities. The gaseous mixture and barometric height being the same as in the last experiment, and the silver plate made positive, when the platinum wire sealed in glass was brought near the plate, and the discharges passed, a spot similar to fig. 8 was formed; but when the coated point of platinum was brought over the copper wire at 0·02 inch distance, a figure consisting of two separated semicircles was formed, having spots in the bisection of the chords, as shown at fig. 10, the portion between the spots and the semicircular line of oxide being of polished silver. With the glass thread the effect was the same, but produced with greater difficulty and not so well defined.

In many repetitions of these experiments which I have made, I have invariably produced the alternately polished and oxidated rings from the bare wire, and have not procured them from the coated wire, except to a very slight degree, and under certain circumstances, which, as far as I could trace, were as follows:—

1st. When the extremity of the wire was very near the plate, so that it had a sensible magnitude with reference to the intervening space, a slight formation of minute rings could be detected at the commencement of the experiment.

2nd. When the experiment was long continued, or when the coated platinum wire had been used for previous experiments, a set of rings, not consisting of an alternation of oxidated and polished rings, but of annuli of different degrees of oxidation, were formed.

When the experiment is continued for some time, a dark deposit is formed on the glass around the extremity of the platinum wire, giving an extended conducting surface; and this may be the reason why such rings are formed, though these rings, in all the cases which I have observed, differ broadly from the rings formed by the bare needle or wire, not having the interposed spaces of perfectly bright silver; and in all the cases the difference of effect produced by the coated and the bare wire is very marked; in by far the greater number of experiments, when proper precautions are taken, not the slightest formation of rings takes place with the coated wire; with the bare wire, in the gaseous mixture last mentioned, I have always seen them formed.

Thus there are three systems of rings which may be formed by the discharge. First, rings such as those seen in the ordinary cases of thin plates; these I have only observed with olefiant gas, though probably there are many other conditions in which they may be produced. Secondly, rings formed by the superposition of layers of oxides, possibly arising from the fact that at certain definite periods portions of the plate become by oxidation inferior conductors, and other portions are attacked, and being at a different distance undergo a different molecular change by oxidation. Thirdly, and to me far the most interesting set of phenomena are presented by the rings alternately bright and oxidated, showing effects of oxidation and reduction by the same current on the same plate, and which only take place in certain gaseous mixtures, of which, up to this time, one volume oxygen+five volumes hydrogen is the most efficient which I have obtained.

I cannot at present see any better mode of explaining these phenomena than by regarding them as analogous to the phenomena of interference in light, though doubtless if this be a right view, the very different modes of action of light and electricity would present very numerous phenomenal distinctions. Alternations of opposite polar electrical actions in the discharges passing in the same direction are, I think, very clearly shown in these experiments, and this appears to me a result worthy of attention.

Though acquainted with Nobili's beautiful experiments on the formation of coloured rings by deposition in electrolyzed liquids, yet as I was working on gases it did not occur to me to refer to his memoirs*; I have done so since making the experiments given in this Postscript, and find that with regard to the rings so formed by electrolysis, he suggests interference as a possible explanation.

The dark space in the discharge to which Faraday has called attention, may possibly be connected with these phenomena. I have observed, that in a well-exhausted receiver containing a small piece of phosphorus, the discharge is throughout its course striated by transverse non-luminous bands, presenting a very beautiful effect, and a yellow deposit, which, as far as I have yet examined it seems to be allotropic phosphorus, is deposited on the plate of the air-pump and on the neigh-

* Ann. de Ch. et de Phys. vol. xxxiv.

bouring substances; to show this effect well the needle should be positive and the plate negative, and the distance between them about an inch.

I could dilate much further on these experiments, but have already trespassed perhaps too far for a Postscript. Variations in the form of the terminals, in the nature of the gas, vapour, or gaseous mixture, in the density of the gas, in the intensity and quantity of the discharge, in the nature of the plate, &c. will occur to those who may feel inclined to repeat these experiments, and if I am not over-sanguine, promise results of much interest.

Description of Plate.

PLATE VIII.

Figures 1 to 10 show the spots and rings in the order referred to: it should be observed that printed figures give but a very imperfect notion of the actual effects.

Fig. 11 is the coil apparatus, the contact breaker being in front.

Fig. 12. The air-pump, of a construction which I proposed many years ago, and have found most useful for electrical or chemical experiments on gases.

P. An imperforate piston, with a conical end, which, when pressed down, fits accurately the end of the tube, the apex touching the valve V, which opens outwards.

A. Aperture for the air to rush from the receiver when the piston has been drawn beyond it.

B. Bladder containing the gas to be experimented on.

The piston-rod works air-tight in a collar of leathers, and the operation of the pump will be easily understood without further description.

If it be required to examine the gas after experiment, a bladder, or tube leading to a pneumatic trough, can be attached at the extremity over the valve V.

VIII. *On Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances.*—No. II. *By* Colonel Edward Sabine, *R.A., Treas. and V.P.R.S.*

Received March 18,—Read May 6, 1852.

In a former paper presented to the Royal Society in January 1851, bearing the same title as the present, I submitted to the Society the evidence afforded by the principal disturbances of the Magnetic Declination at Toronto and Hobarton in the years 1843, 1844 and 1845, of the existence of periodical laws by which their occurrence and mean effects appeared to be regulated. At the close of that paper I expressed the intention of submitting, on some future occasion, the results of a similar investigation into the periodical laws which might be expected to show themselves in like manner in the disturbances of the other two magnetic elements, viz. the Inclination and the Total Force.

Having since had occasion to examine the disturbances of the Declination at the same two stations in the three succeeding years 1846, 1847 and 1848, I have had the satisfaction of finding that the observations of these years confirm every deduction which I had ventured to make from the analysis of the disturbances of the former period; whilst new and important features have presented themselves in the comparison of the frequency and amount of the disturbances in *different years*, apparently indicating the existence of a *periodical variation*, which, either from a causal connection (meaning thereby their being possibly joint effects of a common cause), or by a singular coincidence, corresponds precisely both in period and epoch, with the variation in the frequency and magnitude of the solar spots, recently announced by M. Schwabe as the result of his systematic and long-continued observations. As facts and collocations of this description are of particular interest at the present moment, from their bearing on inquiries in which physical philosophers are engaged, I have deemed it better to communicate them at once, in the form of a second paper on the disturbances of the Declination, than to await the completion of the investigation into the laws of the disturbances of the Inclination and of the Total Force, for which I have not yet been able to command the necessary leisure.

The method pursued in examining the laws of the Declination-disturbances in 1846, 1847 and 1848, is the same as that adopted for the three preceding years and described in my former paper. The observations having been made hourly, every hourly observation which was found to differ by a certain prescribed amount* from the mean value of the Declination in the same month and at the same hour was separated from

* Philosophical Transactions, Part I. 1851, p. 127.

the rest, and a body of *disturbed observations* was thus collected, of which the recognised characteristic was simply that they were the disturbances of largest amount occurring in the whole period. The number of observations thus separated in the period commencing July 1, 1843 and ending July 1, 1848*, was at Toronto 3940, and at Hobarton 3469, being respectively 1 in 9·43 at Toronto and 1 in 10·55 at Hobarton of the whole number of hourly observations. The disturbed observations being next distributed into the several hours, months, and years in which they had occurred, their numbers and aggregate values in each particular hour, month and year, were ascertained. They were then divided into easterly and westerly deflections, and the same process of distribution was gone through with each of the two divisions. The *mean* hourly, monthly, and yearly numbers and aggregate values in the whole period were then taken as the respective units, and the ratios to these units computed for each of the hours, months and years; whereby the relations, whether of numbers or of aggregate values in different hours, different months, and different years, were shown. The numerical values of this analysis will be found in the second volumes of the Toronto and Hobarton Observations, and the ratios only are stated here, as it is only the relation *inter se* of the numbers and values, and not the absolute numbers and values themselves, which are required in this discussion.

I shall proceed to discuss separately,—1st, the Inequality or variation in the occurrence and aggregate values of the disturbed observations in *different hours of the day and night*; 2nd, *in different months of the year*; and 3rd, *in different years*.

I. Inequality or variation in the number and aggregate values of the disturbed observations in *different hours*.

This examination may be most conveniently made, by separating the disturbed observations at once into easterly and westerly disturbances, and classing together,—1st, easterly disturbances at Toronto and westerly at Hobarton; and 2nd, westerly at Toronto and easterly at Hobarton: an appropriate classification, as the stations are in opposite hemispheres, and one which will be seen to be justified by the characteristics which they respectively present.

The following Table exhibits the ratios of the numbers and aggregate values, at the different hours, in the five years from July 1843 to July 1848, of the easterly disturbed observations at Toronto and westerly at Hobarton, to the mean hourly number and aggregate value of the same taken as the respective units.

* The period for which the disturbances were examined in my former paper, was from January 1843 to December 1845 inclusive, making three complete years. For the present paper I have taken from July 1843 to June 1848 inclusive, making five complete years, of which 2½ are part of the period previously examined, and 2½ are new.

TABLE I.

Hours of local astronomical time.	Toronto.		Hobarton.		Hours of local astronomical time.
	Numbers.	Values.	Numbers.	Values.	
h					h
18	0·86	0·69	0·54	0·44	18
19	0·83	0·61	0·73	0·53	19
20	0·82	0·62	0·86	0·70	20
21	0·81	0·59	0·73	0·55	21
22	0·82	0·60	0·77	0·55	22
23	0·81	0·65	0·80	0·62	23
0	0·61	0·45	0·87	0·65	0
1	0·57	0·44	0·87	0·64	1
2	0·44	0·34	0·92	0·71	2
3	0·38	0·33	0·76	0·56	3
4	0·52	0·43	0·73	0·56	4
5	0·54	0·50	0·67	0·52	5
6	0·78	0·94	0·66	0·72	6
7	1·10	1·17	0·94	1·04	7
8	1·31	1·58	1·23	1·31	8
9	1·88	2·47	1·54	1·79	9
10	1·67	1·95	1·72	1·96	10
11	1·61	1·66	1·72	2·31	11
12	1·59	1·57	1·71	2·05	12
13	1·49	1·60	1·48	1·72	13
14	1·17	1·30	1·23	1·52	14
15	1·18	1·25	1·11	1·26	15
16	1·10	1·29	0·82	0·84	16
17	1·09	0·98	0·54	0·47	17

On examining this Table, we perceive,—

1st. That in this division of the disturbed observations, namely, those which are easterly at Toronto and westerly at Hobarton, there is a well-marked and systematic variation dependent on hour in the numbers of the disturbed observations and in their aggregate values. The most obvious feature is, that at both stations there are fewer disturbances, and their aggregate values are less in the hours of the day than in those of the night; and as the stations differ from each other nine hours in longitude, the dependence of the variation is on the hours of *local* and not on those of *absolute* time. If we divide the twenty-four hours at each station into four equal divisions according to the hours of local time, viz. 6 to 11 A.M., noon to 5 P.M., 6 P.M. to 11 P.M., and midnight to 5 A.M., the 1st and 2nd divisions being those of the day, and the 3rd and 4th those of the night, we find the mean ratios to be as follows,—the mean hourly number, and the mean hourly aggregate value in the whole period being still considered as the respective units.

TABLE II.

Hours of local time.		Toronto.		Hobarton.	
		Numbers.	Values.	Numbers.	Values.
Day ...	6 to 11 A.M. inclusive	0·82	0·63	0·74	0·57
	Noon to 5 P.M. inclusive ...	0·51	0·41	0·80	0·61
Night	6 P.M. to 11 P.M. inclusive...	1·40	1·63	1·30	1·52
	Midnight to 5 A.M. inclusive	1·27	1·33	1·15	1·31

2nd. With this marked and very striking correspondence in the diurnal variation of this branch of the larger disturbances at Toronto and Hobarton, both as respects frequency of occurrence and comparative value, we notice minor distinctive features, which, considering the number of years embraced in the inquiry and the systematic mode of observation pursued, may claim to be regarded as indications of persistent rather than of accidental differences. Thus 9 P.M. is at Toronto the hour of maximum frequency and value; both which maxima take place at Hobarton at 11 P.M., or two hours later. This feature is a well-marked one at both stations, and particularly in the aggregate values. In the period of the occurrences of the minimum of frequency and of value there is also a systematic difference, the period at Toronto being 2 and 3 P.M., and at Hobarton 5 and 6 A.M. The features of minimum are however less distinctly marked than those of maximum. As both the numbers and the aggregate values diminish concurrently, it is obvious that the minimum is ascribable chiefly to the diminished *frequency* of the disturbances at those hours; at Toronto 2 and 3 P.M. have decidedly the fewest easterly disturbances, and at Hobarton 5 and 6 A.M. as decidedly the fewest westerly.

It may be useful occasionally to bring into notice, concurrently with the variations of the numbers and aggregate values, the variation of the *average* values of the disturbed observations. The average values at the several hours are the quotients obtained by dividing the aggregate values by the numbers, and the average value in the twenty-four hours constitutes the unit of the ratios which show the variation at the different hours. These ratios, for the easterly disturbances at Toronto and westerly at Hobarton, are contained in Table III.

TABLE III.

Hours of local astronomical time.	Toronto. Easterly.		Hobarton. Westerly.		Hours of local astronomical time.	Toronto. Easterly.		Hobarton. Westerly.	
h					h				
18	0·85		0·87		6	1·26		1·16	
19	0·78		0·77		7	1·15		1·18	
20	0·80	0·81	0·85	0·81	8	1·23	1·23	1·14	1·22
21	0·77		0·80		9	1·39		1·23	
22	0·78		0·75		10	1·23		1·20	
23	0·85		0·82		11	1·09		1·43	
0	0·78		0·77		12	1·05		1·28	
1	0·81		0·77		13	1·13		1·23	
2	0·83	0·87	0·82	0·79	14	1·17	1·11	1·30	1·21
3	0·89		0·77		15	1·12		1·18	
4	0·89		0·82		16	1·23		1·09	
5	0·99		0·80		17	0·95		1·20	

It is here seen that the average value has a similar law of variation to that of the numbers and aggregate values: it is uniformly less during the hours of the day than in the hours of the night; and has a maximum at Toronto at 9 P.M. and at Hobarton at 11 P.M. The epoch of minimum is not strongly marked at either station.

We now pass to the westerly disturbances at Toronto and easterly at Hobarton,—and in Table IV. the ratios of the numbers and aggregate values are arranged opposite to the respective hours; the mean hourly number and aggregate value being taken as the respective units.

TABLE IV.

Hours of local astronomical time.	Toronto.		Hobarton.		Hours of local astronomical time.
	Numbers.	Values.	Numbers.	Values.	
h					h
18	1·17	1·53	0·94	1·02	18
19	1·27	2·16	1·33	1·53	19
20	1·46	1·87	1·41	1·58	20
21	1·39	1·69	1·41	1·41	21
22	1·40	1·33	1·30	1·27	22
23	1·27	1·04	1·32	1·24	23
0	1·10	0·89	1·23	1·14	0
1	0·89	0·66	1·38	1·26	1
2	0·75	0·64	1·38	1·32	2
3	0·99	0·74	1·35	1·40	3
4	0·96	0·76	1·30	1·39	4
5	0·91	0·74	1·16	1·32	5
6	0·75	0·63	1·01	1·16	6
7	0·63	0·51	0·64	0·62	7
8	0·61	0·46	0·45	0·40	8
9	0·60	0·48	0·29	0·32	9
10	0·48	0·54	0·36	0·28	10
11	0·66	0·52	0·88	0·74	11
12	0·73	0·81	0·74	0·62	12
13	0·99	1·10	0·64	0·55	13
14	1·21	1·08	0·70	0·63	14
15	1·23	1·06	0·86	0·85	15
16	1·38	1·25	1·09	1·07	16
17	1·19	1·51	0·67	0·87	17

On examining this Table, we perceive that in this division of the larger disturbances at Toronto and Hobarton, viz. those which are westerly at Toronto and easterly at Hobarton, there is also a very marked and systematic variation dependent upon the hours of local time, but the correspondence of the variation at similar hours of local time at the two stations is not so complete as in the former case. At Hobarton, the contrast both in frequency and aggregate value is still between the hours of the day and those of the night; at Toronto it is between the hours from noon to midnight, and those from midnight to noon. At Hobarton, the *nodal* hours, if they may be so called, are, both in the westerly and easterly disturbances, about 6 A.M. and 6 P.M.; at Toronto they are in the easterly disturbances, about 6 A.M. and 6 P.M.; but in the westerly, six hours different, or about noon and midnight. At Hobarton, in the westerly disturbances the ratios are greater than unity in the night hours, or from 6 P.M. to 6 A.M., and less than unity in the day hours, or from 6 A.M. to 6 P.M.; whilst in the easterly disturbance the converse takes place, the ratios being greater than unity in the day hours, and less than unity in the night hours. At

Toronto, in the easterly disturbances the ratios are greater than unity in the night hours, or from 6 P.M. to 6 A.M., and less than unity in the day hours, or from 6 A.M. to 6 P.M.; but in the westerly disturbances the ratios are greater than unity from midnight to noon, and less than unity from noon to midnight. Dividing the twenty-four hours into four six-hourly divisions as before, we have the mean ratios as follows:—

TABLE V.

Hours of local time.		Toronto.		Hobarton.	
		Numbers.	Values.	Numbers.	Values.
Day ...	6 to 11 A.M. inclusive	1·33	1·60	1·29	1·34
	Noon to 5 P.M. inclusive ...	0·93	0·74	1·30	1·31
Night	6 P.M. to 11 P.M. inclusive...	0·62	0·52	0·61	0·59
	Midnight to 5 A.M. inclusive	1·12	1·13	0·78	0·77

By means of this Table we may perceive more immediately, and therefore perhaps more distinctly, the respects in which the westerly disturbances at Toronto and the easterly at Hobarton agree, and those in which they differ. They agree in the ratios of frequency and aggregate value being above unity from 6 A.M. to noon, and below unity from 6 P.M. to midnight. They differ in the ratios being at Toronto above, and at Hobarton below unity, from midnight to 5 A.M., and at Toronto below, and at Hobarton above unity, from noon to 5 P.M.

The hours of maximum, 7 and 8 A.M., are the same at both stations; as are the hours of minimum, 8 to 10 P.M.

The ratios of the *average* values in this division of the disturbances are as follows:—

TABLE VI.

Hours of local astronomical time.	Toronto. Westerly.	Hobarton. Easterly.	Hours of local astronomical time.	Toronto. Westerly.	Hobarton. Easterly.
h			h		
18	1·33	1·09	6	0·84	1·14
19	1·75	1·17	7	0·84	0·98
20	1·31	1·12	8	0·78	0·87
21	1·40	1·01	9	0·82	1·09
22	0·97	0·98	10	1·14	0·77
23	0·84	0·96	11	0·81	0·85
0	0·84	0·93	12	1·13	0·85
1	0·76	0·90	13	1·14	0·87
2	0·88	0·96	14	0·91	0·90
3	0·78	1·04	15	0·88	0·98
4	0·81	1·06	16	0·93	0·98
5	0·84	1·14	17	1·31	1·33

Here also there is a partial accord and a partial difference between the stations. The ratios are above unity at both stations from 5 to 9 A.M. inclusive; they are also above unity at Toronto at midnight, at 10 P.M. and 1 A.M., and at Hobarton from

3 to 6 P.M. inclusive. On the whole, the average magnitude of a disturbance is greatest at both stations from 5 to 7 A.M.

The preponderance of easterly or westerly aggregate values at the different hours, shows the direction in which the magnet was deflected by the disturbances at the respective hours from the position in which it would have been found had they not occurred; and the ratio of preponderance shows the relative magnitude of the deflection. If we take the aggregate values of the westerly disturbances at Toronto and of the easterly at Hobarton at the different hours as the respective units at those hours, and compute the ratios which the easterly at Toronto and westerly at Hobarton at the same hours bear to them respectively, we obtain, as in the following Table, the ratios which at Toronto the aggregate values of the easterly disturbances bear to the westerly, and at Hobarton the westerly to the easterly, at the different hours of the day and night. When the ratios at Toronto are below unity, the mean deflection of the north end of the magnet at that hour is to the west, and when above unity, to the east. At Hobarton, the ratios which are less than unity indicate easterly deflections, and when above unity, westerly deflections.

TABLE VII.

Hours of local astronomical time.	Toronto. Ratios of easterly aggregate values to westerly.	Hobarton. Ratios of westerly aggregate values to easterly.	Hours of local astronomical time.	Toronto. Ratios of easterly aggregate values to westerly.	Hobarton. Ratios of westerly aggregate values to easterly.
h			h		
18	0·52	0·61	6	1·75	0·86
19	0·32	0·48	7	2·64	2·32
20	0·38	0·61	8	3·93	4·59
21	0·36	0·54	9	5·96	7·87
22	0·52	0·60	10	4·18	9·77
23	0·72	0·69	11	3·71	4·33
0	0·58	0·79	12	2·24	4·58
1	0·76	0·71	13	1·67	4·83
2	0·61	0·75	14	1·38	3·35
3	0·49	0·55	15	1·36	2·07
4	0·66	0·56	16	1·18	1·09
5	0·78	0·55	17	0·75	0·75

We have in this Table unmistakeable evidence of a variation, depending on the hour of local time, in the magnetic direction occasioned by the disturbances, and of a correspondence in the phenomena at the two stations indicative of a common law. During the hours of the day, or from 5 A.M. to 5 P.M. at Toronto and Hobarton, the deflection of the north end of the magnet occasioned by the disturbances is to the west at Toronto, and to the east at Hobarton. A little before 6 P.M. at Toronto, and a little after 6 P.M. at Hobarton, the deflections at both stations pass through zero, (or the undisturbed position of the magnet,) into deflections of the opposite character, becoming easterly at Toronto, and westerly at Hobarton. The magnitude of those deflections rapidly augments to a maximum, which is reached at Toronto at 9 P.M., and at Hobarton an hour later, from which hour it progressively diminishes

until between 4 and 5 A.M., when the deflections at both stations again pass through zero to a maximum of westerly deflection at Toronto, and of easterly at Hobarton, which occur at the same hour, 7 A.M., at both stations.

The mean ratios in each of the four divisions of the twenty-four hours are as follows:—

TABLE VIII.

Hours of local time.		Aggregate values.	
		Toronto. Easterly to Westerly.	Hobarton. Westerly to Easterly.
Day ...	6 A.M. to 11 A.M. inclusive...	0·44	0·59
	Noon to 5 P.M. inclusive ...	0·65	0·65
Night	6 P.M. to 11 P.M. inclusive...	3·70	4·96
	Midnight to 5 A.M. inclusive	1·43	2·78

Passing now for the moment, and in this particular case only, from ratios to *absolute values*, I have placed in the following Table the arc-values of the deflections of the north end of the magnet at Toronto and Hobarton, at the different hours, caused by the disturbed observations, and taken on a daily average during the whole period of observation.

TABLE IX.

Hours of local astronomical time.	Mean diurnal deflection caused by the disturbed observations.		Hours of local astronomical time.	Mean diurnal deflection caused by the disturbed observations.	
	Toronto.	Hobarton.		Toronto.	Hobarton.
h	′	′	h	′	′
18	0·24 West.	0·06 East.	6	0·14 East.	0·02 East.
19	0·48 West.	0·13 East.	7	0·28 East.	0·13 West.
20	0·37 West.	0·10 East.	8	0·46 East.	0·23 West.
21	0·33 West.	0·11 East.	9	0·78 East.	0·35 West.
22	0·21 West.	0·09 East.	10	0·56 East.	0·40 West.
23	0·10 West.	0·07 East.	11	0·46 East.	0·40 West.
0	0·13 West.	0·04 East.	12	0·33 East.	0·36 West.
1	0·05 West.	0·07 East.	13	0·24 East.	0·29 West.
2	0·08 West.	0·05 East.	14	0·14 East.	0·24 West.
3	0·12 West.	0·10 East.	15	0·13 East.	0·15 West.
4	0·08 West.	0·09 East.	16	0·08 East.	0·01 West.
5	0·05 West.	0·10 East.	17	0·13 West.	0·03 East.

The analogy of the two stations is generally so close as to give a greater importance than might otherwise be ascribed to the principal feature of difference in the diurnal progression, namely, that the nocturnal easterly deflection at Toronto precedes the westerly at Hobarton throughout by about an hour; it commences earlier, reaches its maximum earlier, and diminishes earlier.

II. Inequality or Variation in the numbers and values of the disturbed observations in *different months*.

The following Tables X. and XI. show the ratios of the numbers and aggregate values of the disturbed observations at Toronto and Hobarton in different months to the mean monthly number and aggregate value taken as the respective units.

TABLE X.—Toronto.

Months.	Easterly.		Westerly.		Easterly and Westerly combined.	
	Numbers.	Values.	Numbers.	Values.	Numbers.	Values.
January	0·60	0·59	0·71	0·60	0·65	0·59
February ...	0·69	0·78	0·81	0·80	0·74	0·79
March	1·02	1·09	1·02	0·98	1·02	1·04
April	1·20	1·31	1·27	1·38	1·24	1·34
May	1·10	1·03	0·99	0·94	1·05	0·99
June	0·88	0·77	0·75	0·52	0·82	0·66
July	1·22	1·12	1·01	0·88	1·12	1·01
August	1·42	1·37	1·29	1·08	1·36	1·24
September ...	1·65	1·63	1·47	1·74	1·57	1·69
October	1·01	1·15	1·09	1·19	1·04	1·17
November ...	0·66	0·64	0·85	0·83	0·75	0·73
December ...	0·55	0·53	0·74	1·05	0·64	0·77

TABLE XI.—Hobarton.

Months.	Westerly.		Easterly.		Westerly and Easterly combined.	
	Numbers.	Values.	Numbers.	Values.	Numbers.	Values.
January	1·82	1·54	1·65	1·62	1·74	1·58
February ...	1·16	1·05	1·21	1·16	1·18	1·10
March	1·04	1·11	1·14	1·11	1·08	1·12
April	1·02	1·18	1·10	1·26	1·05	1·22
May	0·53	0·51	0·62	0·65	0·57	0·57
June	0·37	0·32	0·32	0·30	0·35	0·31
July	0·47	0·54	0·50	0·51	0·49	0·53
August	0·78	0·73	0·86	0·84	0·82	0·78
September ...	1·14	1·50	1·35	1·29	1·24	1·41
October	1·23	1·27	1·24	1·22	1·24	1·25
November ...	1·11	0·95	0·79	0·73	0·97	0·86
December ...	1·30	1·29	1·23	1·29	1·27	1·29

It is obvious, on the mere inspection of these Tables, that there is a systematic variation in the numbers and aggregate values of the disturbances in the different months; and as at both stations the easterly and westerly ratios, separately considered, differ little, in the characters which they assign to the variation, from the ratios of the two combined, we may fix our attention chiefly on the two final columns of each table.

The most distinctly marked feature is that the disturbances are less frequent, and have a less aggregate value in November, December, January and February at Toronto,

and in May, June, July, and August at Hobarton than in the other eight months of the year. As we have before seen that in the *hours* of their occurrence the disturbances appear to be governed by a law depending on *local* hours, so here, we recognise *local* effects depending on the period of the year, and possibly the influence of *local* seasons (since we are scarcely yet in a condition to discriminate as to causes). The mean monthly ratios in the different seasons are shown in the following Table, in which it will be remembered that November, December, January and February, are the winter months at Toronto, and May, June, July and August, at Hobarton.

TABLE XII.

Stations.	Winter.		Summer.		Spring and Autumn.	
	Numbers.	Values.	Numbers.	Values.	Numbers.	Values.
Toronto	0·70	0·72	1·08	0·96	1·22	1·31
Hobarton ...	0·56	0·56	1·28	1·21	1·16	1·24

It is seen that the mean ratios, both of frequency and of aggregate value, are much less in the winter months at both stations than in the months of summer or of spring and autumn, or, otherwise stated, least at Toronto in the months when the sun is in the southern signs, and least at Hobarton in the months when he is in the northern signs.

If we compare the ratios of the numbers in the different months with those of the aggregate values in the same months, we perceive that the *average* value of a disturbed observation is greater at both stations in the winter than in the summer months, and that it is greatest in the intermediate or equinoctial months. In these respects, and also in the circumstance of the minimum ratio, both of numbers and aggregate values, being in the midwinter month, (January at Toronto, and June at Hobarton,) the two stations agree. They differ 1° in the relative amount of the mean ratios in the months of summer and in those of spring and autumn, the mean ratios being decidedly greater at Toronto in spring and autumn than in summer, whilst at Hobarton there is no such marked difference: and 2° in the character of the progression which the ratios at the two stations indicate in the different months: at Toronto there are two decided minima, one in the midwinter, and the other in the midsummer, with a progressive increase on either side of the respective minima to April and September, which are the months of maximum disturbance: at Hobarton there is but one decided minimum, which is in the midwinter month; whilst in January, which is the month of midsummer, there is as decided a maximum in the ratios, both of numbers and values. At the same time there is so far an agreement with Toronto, that there is a tendency at Hobarton towards secondary maxima in April and September.

The points of accordance and points of difference thus noticed, are precisely the

same as were deduced in my former paper from the observations in 1843, 1844 and 1845. From the confirmation which has now been derived from the observations in the following years, 1846, 1847 and 1848, they may perhaps claim to be regarded as the exponents of persistent natural laws.

III. Variation in the numbers and aggregate values of the disturbed observations *in different years*.

Table XIII. exhibits the ratios of the numbers and aggregate values of the disturbed observations at Toronto and Hobarton in the different years, to the average annual number and aggregate value respectively*.

TABLE XIII.

Years.	Numbers.		Values.		Years.
	Toronto.	Hobarton.	Toronto.	Hobarton.	
1843.....	0·68	0·52	0·55	0·48	1843.
1844.....	0·76	0·81	0·73	0·82	1844.
1845.....	0·72	0·72	0·62	0·67	1845.
1846.....	1·31	1·09	1·26	1·03	1846.
1847.....	1·19	1·36	1·40	1·44	1847.
1848.....	1·37	1·50	1·43	1·60	1848.

On the first aspect of this Table, two features of principal interest present themselves; first, there is a considerable variation in the numbers and values of the disturbed observations in different years; and second, there is a remarkable correspondence in the variation in different years at the two stations.

Before we proceed to consider the first of these features, which is obviously one of great importance, it may be desirable, in reference to the correspondence at the two stations, to state more precisely than has been done previously, the degree of simultaneity at the two stations of the observations from which the conclusions are derived.

The weeks of observation commenced at both stations at midnight on the Sundays, and terminated an hour before midnight on the Saturdays. As these epochs were of *local* time, the week of observation commenced at Hobarton at 3^h, and at Toronto at 18^h of Göttingen time of the same astronomical day; and terminated at Hobarton at 2^h, and at Toronto at 17^h of Göttingen time. There were consequently fifteen hours at the commencement of each week (3^h to 17^h Göttingen) in which observations were made at Hobarton without simultaneous observations at Toronto; and fifteen hours at the termination of each week in which observations were made at Toronto without simultaneous observations at Hobarton. There were also nine hours in every week, between the conclusion of the one week at Toronto and the

* As 1843 and 1848 are only half years, the ratios are taken to the half average annual number and half average aggregate value in the five years.

commencement of the next at Hobarton (18^h at Göttingen to 2^h of the following day), in which no observations were made at either observatory.

In a week of seven days there are 168 hours, and deducting nine, there remain 159 hours in each week in which observations were made at one or other of the two observatories, from which the ratios in Table XIII. are derived. Deducting twice fifteen or thirty hours from the 159, we have 129 hours in each week in which observations were made simultaneously at the two observatories, and thirty hours in which they were made at one or other observatory, but not simultaneously at both. A still further small deduction would require to be made from the simultaneous portion, on account of the Good Fridays and the Christmas days, and of the observations accidentally missed. We may state, therefore, in round numbers, that about four-fifths of the whole number of observations which have contributed to form the ratios in Table XIII. were simultaneous at both stations, and that about one-fifth were not so.

This consideration being premised, we shall be inclined perhaps to regard the accordance in the ratios at the two stations in different years as being quite as near as could be expected, even on the extreme supposition which the case will admit, namely, that of *all* disturbances being *general*. That they are so *for the most part* at Toronto and Hobarton, may be concluded from the circumstance, that by far the greater part of the disturbances which form the subject of discussion in this paper, occurred *on the same days at the two stations*. This may be seen by comparing the Tables in the Abstracts prefixed to the second volumes of the Toronto and Hobarton Observations, in which the 3940 largest disturbances at Toronto, and the 3469 at Hobarton, are placed in separate tables, showing the day and hour of the occurrence, together with the direction and amount of each, for the purpose of facilitating their intercomparison, and of aiding generally in comparisons of a similar nature between the observations at these stations and the observations which by concerted arrangement may have been made simultaneously with them at other observatories. In all such comparisons the modifying influence of hours and periods of the year shown in the first and second sections of this paper, must be kept in view; and it must also be remembered that the evidence of the *general* character of these magnetic affections, which may be afforded by the comparison of the observations of the Declination alone, may be expected to be greatly strengthened when the disturbances of the Inclination and of the Total Force shall have been subjected to a similar process. The evidence furnished by a single element must necessarily be partial and incomplete.

Recurring now to the ratios in Table XIII., and directing our attention to the *character* of the inequality which they show to have existed in the amount of disturbance in different years, the facts which present themselves most obviously and unquestionably to our notice are, that in the years 1843, 1844 and 1845, the ratios were uniformly *considerably less than unity*, and that in the years 1846, 1847 and 1848, they were as uniformly *considerably greater than unity*. The mean ratios

in the three first years are, at Toronto 0·69, and at Hobarton 0·68; whilst in the three last years they are at Toronto 1·33, and at Hobarton 1·34. Facts of such remarkable character, evidenced by the independent and concurrent testimony of so large a body of observations at stations so widely distant from each other, seem to be well deserving the consideration of magnetical physicists; more particularly of those who are disposed to regard thermometrical differences as the cause of the periodical and other magnetic variations. The ratios of disturbance in the years 1846, 1847 and 1848, were nearly *twice as great* as in the years 1843, 1844 and 1845. Did there occur any notable differences of either local or general temperature, or thermometrical peculiarities of any description, in the years in question, to which variations of such magnitude in the ratios of magnetic disturbance can be ascribed, or with which they can be connected?

We should not however derive all the advantage which an examination of the ratios in Table XIII. seems suited to afford to those who desire to obtain an insight into the character of the variations they represent, were we to overlook the still more remarkable fact which they manifest, of a general, and with a single exception, uninterrupted *progressive increase* in the amount of disturbance from a minimum in 1843 to a maximum in 1848.

The interruption is in 1845, when the ratios, both of numbers and values, are less than in 1844. This interruption of the perfect continuity of the progression occurs alike at both stations; it is not of large amount, and is the sole exception to an otherwise continuous increase in the amount of disturbance during the years comprehended in this investigation.

The accordance with which the ratios at Toronto and Hobarton indicate this progression, is scarcely less remarkable than are the facts which they combine to indicate. It is indeed difficult to regard results so strikingly correspondent in any other light, than as independent and mutually corroborative measures of the same general phenomenon; and to view the inconsiderable differences between the ratios of the several years at the two stations as due either to accidents of observation, or to the want of strict simultaneity in all cases which has already been described. In such case a combination of the ratios obtained in opposite hemispheres would perhaps present a not improbable approximate view of the general variation in the amount of disturbance in the different years, occasioned in both hemispheres by the class of phenomena under notice. It is contained in the following Table.

TABLE XIV.—Mean of the ratios at Toronto and Hobarton.

Years.	Numbers.	Values.
1843.....	0·60	0·52
1844.....	0·78	0·78
1845.....	0·72	0·65
1846.....	1·20	1·15
1847.....	1·28	1·42
1848.....	1·43	1·51

The variation in the amount of disturbance in the different years presented in this Table, has certainly far more the aspect of a *periodical inequality*, than of what may be called for distinction's sake, *accidental* variation. The character, with the single exception already noticed, is that of an increase systematically progressive between the years 1843 and 1848. But the existence of a periodical inequality of this nature, affecting at the same time, and in the same manner, parts of the globe most remote from each other, would be a circumstance of such extreme importance in theoretical respects, that we are bound to receive the facts which may appear to indicate it with the utmost caution, and to await the confirmation it may obtain from contemporaneous observations at other stations. The magnetic disturbances present as well-marked and as notable features over the greater part of Europe as they do at Toronto and Hobarton; and there exist, or there should exist at those European observatories which have professed to adopt and carry out the system of observation proposed by the Royal Society, hourly or two-hourly observations, not only contemporaneous but simultaneous with those which have been discussed in this paper, and which, if examined, should yield corresponding conclusions, if the phenomena be general. In magnetical no less than in astronomical observations, the work of an observatory is but partially performed, until the observations have been subjected to processes of reduction, and their bearing on the points of theory for which they were instituted has been examined and shown.

Pending such confirmation, the general progressive increase in the amount of disturbance at Toronto and Hobarton, between the years 1843 and 1848, derives great additional interest and importance from its apparent connection with an equally remarkable progressive increase which took place at the same two stations, in the magnitude of the diurnal range of the Declination in the same years. The Tables in which the hourly observations at Toronto and Hobarton are recorded in the volumes of those observatories, exhibit for each month the mean monthly diurnal variation: the extreme east and west positions of the magnet at any two hours in these monthly means, show the mean magnitude or average range of the diurnal variation in that month. The subjoined Tables XV. and XVI. contain the mean magnitudes or ranges in the four months constituting the respective seasons, and in the twelve months constituting the year, in each year from 1843 to 1848.

TABLE XV.—Mean monthly diurnal range of the Declination at Toronto.

Years.	Winter.	Spring and Autumn.	Summer.	Mean in the whole year.
	November, December, January, February.	March, April, September, October.	May, June, July, August.	
1843.....	5·64	9·36	11·70	8·90
1844.....	5·70	8·74	12·17	8·87
1845.....	5·73	9·15	13·36	9·41
1846.....	6·33	9·21	12·27	9·27
1847.....	7·28	10·08	13·84	10·40
1848.....	9·48	11·04	15·82	12·11

TABLE XVI.—Mean monthly diurnal range of the Declination at Hobarton.

Years.	Winter.	Spring and Autumn.	Summer.	Mean of the whole year.
	May, June, July, August.	September, October, March, April.	November, December, January, February.	
1843.....	4′·50	7′·80	10′·16	7′·66
1844.....	4·30	8·45	10·77	7·84
1845.....	4·39	8·61	12·16	8·39
1846.....	5·10	9·50	12·58	9·06
1847.....	5·38	10·97	13·43	9·93
1848.....	7·09	10·67	14·14	10·63

We perceive by Tables XV. and XVI. that a generally and almost uniformly progressive increase took place at Toronto and Hobarton in the mean monthly range of the diurnal variation of the Declination between 1843 and 1848, contemporaneously with the increase in the amount of the disturbances produced by the variations which are of less regular occurrence and have distinct phenomenal laws. This coincidence appears to afford a more direct and decided indication of a causal connection subsisting between the two classes of phenomena than any which has previously presented itself.

It might be supposed that an increased amount of disturbance occurring in any year from the last named causes, *i. e.* the disturbances, might have a *direct* effect in increasing the diurnal range of the Declination; and such undoubtedly must be the case on individual days; but when *mean* values are in question, as is the case here, the difference in the mean monthly diurnal range, when the disturbed observations are retained, or when they are omitted, is scarcely sensible. The following Table contains the mean monthly diurnal range in the different years at Hobarton, when the 3469 disturbances of largest amount have been withdrawn. When compared with the values in Table XVI., where the disturbances are retained, the difference is seen to be wholly insignificant.

TABLE XVII.—Mean monthly diurnal range of the Declination at Hobarton, the 3469 disturbances of largest amount being omitted.

Years.	Winter.	Spring and Autumn.	Summer.	Mean of the whole year.
	May, June, July, August.	March, April, September, October.	November, December, January, February.	
1843.....	4′·49	7′·80	10′·67	7′·65
1844.....	4·19	8·29	10·62	7·70
1845.....	4·37	8·57	12·13	8·36
1846.....	4·59	9·26	12·76	8·87
1847.....	5·18	10·50	13·80	9·83
1848.....	7·02	10·53	14·19	10·58

Facts so remarkable as those presented in Tables XV. and XVI., showing in the course of six years a progressive increase in the range of the diurnal variation taken

from the monthly means of the observations in the several months, from 8′·90 to 12′·11 at one station, and from 7′·66 to 10′·63 at another station separated from the former by nearly half the surface of the globe, might naturally have created an expectation that they would prove to be independent and corresponding measures of a general phenomenon. Fortunately, in the case of the diurnal range, we have not to wait, as we have in the case of the disturbance-progression, for a confirmation of its extension to Europe. In a recent number of POGGENDORFF's Annalen, 1851, No. 12, December 23 (which only reached the author of this paper when the greater part of it was already written), Dr. LAMONT has published a Table of the mean monthly range of the diurnal variation of the Declination at Munich, from 1841 to 1850 inclusive, from which he also has been led to infer the probable existence of a periodical inequality, having its epoch of minimum in 1843·5, and of maximum in 1848·5. The mean range of the diurnal variation in monthly periods at Munich in the years discussed in this paper, is stated in Dr. LAMONT's communication to have been as follows:—

1843	7′·15
1844	6·61
1845	8·13
1846	8·81
1847	9·55
1848	11·15

The years which Dr. LAMONT infers from the Munich observations to have been those which include the half-period of the inequality of the diurnal range, (or that portion of the period which is comprised between the epochs of minimum and maximum,) are precisely the same years over which my discussion of the disturbances has extended, and from which I have been led to infer the probable existence of a periodical inequality in those phenomena also, having the very same epochs of minimum and maximum. Dr. LAMONT confines himself entirely to the diurnal inequality of the Declination, leaving untouched the subject of the disturbances (or, as they are more usually termed in Germany, the magnetic storms).

Whether the progressive increase so distinctly marked in the two classes of phenomena between the years 1843 and 1848 be or be not the result of causes which have a periodical action, in a cycle which may be either of regular or of variable duration, the fact of the progressive increase being concurrent in both classes is of no slight importance. It tends to indicate a causal connection subsisting between the disturbances and the diurnal variation; which latter, in addition to the laws which point directly or indirectly to the sun as the origin of its every-day phenomena, has other phases which mark unmistakeably, and at stations very variously situated in geographical and magnetical respects, the equinoxes as epochs of periodical change. The investigation, of which the results are contained in this paper, has shown that the disturbances have also a law of diurnal action, depending like that of the regular

diurnal variation on the hours of local time, but with different hours of maxima and minima; it has also shown, generally, that there is an influence connected with the period of the year on the frequency of occurrence of the disturbances of principal magnitude, affecting the aggregate effects of the disturbing causes; but it has not yet succeeded in tracing a definite epoch of semiannual change in the disturbances with the same precision as in the diurnal variation, of which the phenomena have been so much longer and so much more minutely studied, and in which the epoch of change in the phase depending on the earth's revolution in its orbit has been distinctly traced to the very days of the equinoxes*.

The progressive augmentation of the range of the diurnal variation between 1843 and 1848, is quite as conspicuous in the respective winters at Toronto and Hobarton as in their summers: the ratio of the increment of the range is even somewhat greater in winter than in summer. This has an important bearing when we regard the diurnal variation as divided into two portions, one depending on the earth's revolution on its axis, and the other on the earth's revolution in its orbit.

The extreme range of the periodical inequality of the diurnal variation from 1843 to 1848, *i. e.* the difference between the ranges of the diurnal variation in 1843 and 1848, is less at Toronto and Hobarton than the difference in the mean winter and summer ranges in any single year, *i. e.* than the inequality due to the position of the sun with reference to the equator.

That the progressive increase in the mean monthly diurnal range from 1843 to 1848, was not confined at Toronto and Hobarton to the Declination only, but took place likewise in the diurnal variations of the Inclination and Total Force, is shown in the subjoined Tables XVIII. and XIX., XX. and XXI., which appear to require no further explanation, as they are arranged on the same plan as Tables XV. and XVI.

TABLE XVIII.—Mean monthly diurnal range of the Inclination at Toronto.

Years.	Winter.	Spring and Autumn.	Summer.	Mean in the whole year.
	November, December, January, February.	March, April, September, October.	May, June, July, August.	
	′	′	′	
1843.....	1·26	1·40	1·50	1·39
1844.....	0·78	1·39	1·59	1·25
1845.....	0·88	1·35	1·57	1·27
1846.....	1·09	1·59	1·92	1·53
1847.....	1·43	2·22	1·98	1·88
1848.....	1·73	2·17	2·18	2·03

* Magnetical and Meteorological Observations at the Cape of Good Hope, vol. i. p. 15, &c.; and at Toronto, vol. ii. Plate II. figs. 6 and 7.

TABLE XIX.—Mean monthly diurnal range of the Total Force at Toronto, in parts of the Force.

Years.	Winter.	Spring and Autumn.	Summer.	Mean in the whole year.
	November, December, January, February.	March, April, September, October.	May, June, July, August.	
1843.....	·00028	·00040	·00040	·00036
1844.....	·00027	·00059	·00039	·00042
1845.....	·00032	·00038	·00046	·00039
1846.....	·00020	·00059	·00077	·00052
1847.....	·00048	·00064	·00052	·00055
1848.....	·00061	·00075	·00054	·00063

TABLE XX.—Mean monthly diurnal range of the Inclination at Hobarton.

Years.	Winter.	Spring and Autumn.	Summer.	Mean in the whole year.
	May, June, July, August.	September, October, March, April.	November, December, January, February.	
	′	′	′	′
1843.....	1·43	1·68	1·58	1·56
1844.....	1·39	1·76	1·72	1·62
1845.....	1·25	1·78	2·42	1·85
1846.....	1·62	2·04	2·36	2·01
1847.....	1·54	2·45	2·70	2·23
1848.....	1·92	2·40	3·27	2·53

TABLE XXI.—Mean monthly diurnal range of the Total Force at Hobarton, in parts of the Force.

Years.	Winter.	Spring and Autumn.	Summer.	Mean in the whole year.
	May, June, July, August.	September, October, March, April.	November, December, January, February.	
1843.....	·00057	·00043	·00049	·00050
1844.....	·00029	·00033	·00038	·00033
1845.....	·00031	·00031	·00036	·00033
1846.....	·00033	·00038	·00039	·00037
1847.....	·00031	·00040	·00049	·00040
1848.....	·00032	·00039	·00063	·00045

We perceive by these Tables that the indications of a periodical inequality are not less general in magnetical respects, *i. e.* in their extension to all the magnetical elements, than in their manifestation in parts of the globe most remote from each other. In the Inclination, the progressive increase is as distinctly marked as in the Declination; and if it is somewhat less so in the Total Force, this may with some probability be ascribed to the less perfect instrumental means from which the diurnal variation of that element is chiefly derived. In our present ignorance of the physical agency by which the periodical magnetic variations are produced, the possibility of the discovery of some cosmical connection which may throw light on a subject as

yet so obscure, should not be altogether overlooked. As the sun must be recognised as at least the *primary* source of all magnetic variations which conform to a law of local hours, it seems not unreasonable that in the case of other variations also, whether of irregular occurrence or of longer period, we should look in the first instance to any periodical variation by which we may learn that the sun is affected, to see whether any coincidence of period or epoch is traceable. Now the facts of the *solar spots*, as they have been recently made known to us by the assiduous and systematic labours of Schwabe, present us with phenomena which appear to indicate the existence of some periodical affection of an outer envelope, (the photosphere,) of the sun; and it is certainly a most striking coincidence, that the period, and the epochs of minima and maxima, which M. Schwabe has assigned to the variation of the solar spots, are absolutely identical with those which have been here assigned to the magnetic variations. In the third volume of Kosmos, page 402 (English translation, vol. iii. pp. 291 and 292), Baron von Humboldt has published a tabular abstract supplied by M. Schwabe, of the results of that gentleman's observations of the solar spots from 1826 to 1850; from which M. Schwabe has derived the conclusion, that "the numbers in the Table leave no room to doubt that, at least from the years 1826 to 1850, the solar spots have shown a period of about ten years, with maxima in 1828, 1837 and 1848, and minima in 1833 and 1843." "In almost all the years except those of the minima," M. Schwabe says, "I have observed large spots visible to the naked eye,—I mean spots whose diameters are above 50″:" and 1847 and 1848 are enumerated amongst the years in which the largest spots appeared. M. Schwabe's Table is as follows:—

Table XXII.

Years.	Groups of spots.	Days free from spots.	No. of days of observation.
1826.....	118	22	277
1827.....	161	2	273
1828.....	225	0	282
1829.....	199	0	244
1830.....	190	1	217
1831.....	149	3	239
1832.....	84	49	270
1833.....	33	139	267
1834.....	51	120	273
1835.....	173	18	244
1836.....	272	0	200
1837.....	333	0	168
1838.....	282	0	202
1839.....	162	0	205
1840.....	152	3	263
1841.....	102	15	283
1842.....	68	64	307
1843.....	34	149	312
1844.....	52	111	321
1845.....	114	29	332
1846.....	157	1	314
1847.....	257	0	276
1848.....	330	0	278
1849.....	238	0	285
1850.....	186	2	308

M. Schwabe has not been able to derive from the indications of the thermometer or barometer any sensible connection between climatic conditions and the number of spots. The same remark would of course hold good in respect to the connection of climatic conditions with the magnetic inequalities, as their periodical variation in different years corresponds with that of the solar spots. But it is quite conceivable that affections of the gaseous envelope of the sun, or causes occasioning those affections, may give rise to sensible *magnetical* effects at the surface of our planet, without producing sensible *thermic* effects.

It may be confidently anticipated that so remarkable a coincidence in the degree of energy with which the causes producing obscurations in the luminous disc of the sun, and those producing the magnetic variations at the surface of our planet, appear to have acted in the different years between 1843 and 1848, will receive due attention at those observatories which, by their more permanent character, are more particularly adapted for the investigation of problems requiring several years for their solution.

As the physical agency by which the phenomena are produced is in both cases unknown to us, our only resource for distinguishing between accidental coincidence and causal connection seems to be *perseverance in observation*, until either the inferences from a possibly too limited induction are disproved, or until a more extensive induction has sufficed to establish the existence of a connection, although its precise nature may still be imperfectly understood. For such continued investigation we must look to those observatories which are permanent in their institution; and in this particular problem, to those especially which combine astronomical and magnetical research. The hourly observations at the British Colonial Observatories, which, combined with M. Schwabe's observations of the sun in Germany, have led to the discovery of the existence of the coincidence during the years 1843–1848, ceased in 1848, having accomplished the special objects for which they were instituted. There are yet remaining for analysis, in reference to the disturbance-variations, the hourly observations at Hobarton in 1841 and 1842, and the 2-hourly at Toronto in the same years, which will show whether the aggregate values of the disturbances were greater in those years than in 1843, as they should have been in conformity with a periodical inequality having 1843 as a minimum epoch.

Woolwich, March 16, 1852.

Postscript, May 24th, 1852.

The interval which has elapsed between the presentation and the printing of this paper has enabled me to complete the examination of the disturbances at Toronto from the commencement of the observations in 1841, and to subjoin the results. The observations were made two-hourly from January 1841 to June 1842 inclusive, and hourly from July 1842 to July 1848, inclusive: all those which differed 3·6 or upwards from the mean or normal Declination at the same hour in the same month have been separated from the others, and constitute the body of disturbed observations submitted to examination. It appears desirable to state, in addition to the ratios, the numbers and aggregate values of the disturbed observations in each year, dividing them into easterly and westerly disturbances: this is done in the following Table:—

Table XXIII.

Years.	Disturbed observations.					
	Numbers.			Aggregate values.		
	Easterly.	Westerly.	Total.	Easterly.	Westerly.	Total.
1841. Twelve months: observations two-hourly	282	288	570	1865	1750	3615
1842. { January to June, two-hourly / July to December, hourly }	327	279	606	1947	1623	3570
1843. Twelve months; hourly	268	204	472	1515	1133	2648
1844. Ditto Ditto	327	269	596	2162	1692	3854
1845. Ditto Ditto	298	269	567	1761	1544	3305
1846. Ditto Ditto	547	484	1031	3655	3002	6657
1847. Ditto Ditto	532	409	941	3620	3804	7424
1848. January to June, hourly	288	250	538	2185	1609	3794

It will be observed that in this Table the values from 1843 to 1847 inclusive, are strictly intercomparable, those years being complete years of hourly observation; whereas in 1841 and in part of 1842 the observations were only two-hourly, and in 1848 the observations were limited to the first six months. In order therefore to render the numbers and aggregate values in Table XXIII. readily comparable by the eye, it may be convenient to double those of 1841 and 1848, and to augment those of 1842 in the proportion of 4 to 3: this is done in the following Table:—

Table XXIV.

Years.	Numbers.			Aggregate values.		
	Easterly.	Westerly.	Total.	Easterly.	Westerly.	Total.
1841.	564	576	1140	3730	3500	7230
1842.	436	372	808	2596	2164	4760
1843.	268	204	472	1515	1133	2648
1844.	327	269	596	2162	1692	3854
1845.	298	269	567	1761	1544	3305
1846.	547	484	1031	3655	3002	6657
1847.	532	409	941	3620	3804	7424
1848.	576	500	1076	4370	3217	7587

Assuming the existence of a periodical inequality, such as has been described in the paper to which this Postscript is annexed, we may take the means of the numbers and values from 1843 to 1848 inclusive, (constituting approximately the half period of the supposed inequality,) as the units of ratios, which will show with more convenient simplicity the variation of the numbers and aggregate values in the different years. These are contained in Table XXV.

TABLE XXV.

		Numbers.			Aggregate values.		
		Easterly.	Westerly.	Total.	Easterly.	Westerly.	Total.
Units.		424·7	355·8	780·5	284$\frac{7}{10}$	239$\frac{9}{10}$	524$\frac{6}{10}$
Ratios.	1841.	1·33	1·62	1·46	1·31	1·46	1·38
	1842.	1·03	1·05	1·04	0·91	0·90	0·91
	1843.	0·63	0·57	0·60	0·53	0·47	0·50
	1844.	0·77	0·76	0·76	0·76	0·71	0·73
	1845.	0·70	0·76	0·73	0·62	0·64	0·63
	1846.	1·29	1·36	1·32	1·28	1·25	1·27
	1847.	1·25	1·15	1·21	1·27	1·59	1·42
	1848.	1·36	1·40	1·38	1·54	1·34	1·45

It is seen by this Table that the ratios, both of numbers and values, in the years 1841 and 1842 are greater than in 1843, and that 1843 is in every respect, whether of numbers or values, easterly or westerly, the minimum of the whole series. The ratios of the numbers and values in 1841 are greater than in 1842, presenting the same appearance of a periodical inequality previous to 1843, as in the years subsequent to that date.

It may be desirable to subjoin also in one view the inequality in the range of the Diurnal Variation at Toronto and Hobarton from 1841 to 1851 inclusive, and at Munich from 1841 to 1850 inclusive, the latter being taken from the Number of POGGENDORFF'S Annalen, already referred to.

TABLE XXVI.

Years.	Toronto.				Munich.			Hobarton.			
	Nov. Dec. Jan. Feb.	Mar. Apr. Sept. Oct	May, June, July, Aug.	Mean of the whole year.	Winter.	Summer.	Mean of the whole year.	Nov. Dec. Jan. Feb.	Mar. Apr. Sept. Oct.	May, June, July, Aug.	Mean of the whole year.
1841.	6·67	9·46	12·38	9·50	5·12	10·53	7·82	11·13	8·77	4·94	8·28
1842.	5·67	8·87	11·48	8·67	5·07	9·09	7·08	10·56	8·14	4·55	7·75
1843.	5·64	9·36	11·70	8·90	4·70	9·59	7·15	10·16	7·80	4·50	7·66
1844.	5·70	8·74	12·17	8·87	4·44	8·79	6·61	10·77	8·45	4·30	7·84
1845.	5·73	9·15	13·36	9·41	5·89	10·37	8·13	12·16	8·61	4·39	8·39
1846.	6·33	9·21	12·27	9·27	6·08	11·55	8·81	12·58	9·50	5·10	9·06
1847.	7·28	10·08	13·84	10·40	7·13	11·98	9·55	13·43	10·97	5·38	9·93
1848.	9·48	11·04	15·82	12·11	7·85	14·44	11·15	14·14	10·67	7·09	10·63
1849.	8·25	12·25	14·80	11·77	8·06	13·21	10·64	11·32	8·28	4·79	8·13
1850.	8·01	10·90	13·74	10·88	7·61	13·27	10·44	11·32	9·50	4·89	8·57
1851.	7·01	10·82	12·61	10·15	.....	.....	.....	8·54	6·85	4·57	6·65

IX. *On the Lunar Atmospheric Tide at Singapore.*

By Captain C. M. Elliot, *Madras Engineers, F.R.S.*

Received December 18, 1851,—Read March 11, 1852.

At the commencement of the year 1847, a paper by Colonel Sabine, R.A., V.P.R.S., was read before the Royal Society on the Lunar Atmospheric Tide at St. Helena. The influence of the moon upon the barometer, although small in amount, was shown in a very striking and decided manner; for after eliminating the regular diurnal variation, the differences arranged in lunar tables showed a decided maximum, both at the superior and inferior culmination of the moon, and a decided minimum at its rising and setting.

The effect which the moon's position, relatively to the meridian of the place, had upon the barometric pressure, was publicly noticed, about the middle of 1842, by Captain Lefroy, R.A., who appears to have had his attention directed to it from the first establishment of the observatory at St. Helena.

On the receipt of Colonel Sabine's paper, I was anxious to ascertain if the fact of the moon's influence, so clearly and decidedly shown at St. Helena, could be similarly proved by the Singapore barometric observations.

I therefore determined, before leaving England for India, to proceed upon the plan adopted by Colonel Sabine, and in order that a comparison might be made between the results at Singapore and at St. Helena, have copied to a considerable extent the form of the different lunar tables drawn up by him in his very valuable paper.

The observatory at Singapore was in latitude 1° 18′ 32″ north, and longitude 103° 56′ 30″ east of Greenwich. The cistern of the barometer was a few feet above high-water mark: the barometer was made by Newman: the diameter of the tube =0·532 inch. The observations of the barometer, by my assistants, were made at every two hours during the whole of 1841, the early part of 1842, and that of 1843; during the rest of the time, to the close of 1845, at every hour.

In making out the Tables, showing the moon's effect upon the barometer, I have only taken complete astronomical days, from noon to noon; and as Sundays were omitted, and as the observations commenced at midnight on Sunday and terminated at 11 P.M. on Saturday, the broken portions of Monday and of Saturday have not been taken into consideration; the mean, however, of the entire month has been assumed to be identical with the mean of these complete days.

The Barometer Tables, corrected to 32°, having been made out, the mean monthly

height of the barometer at each hour was deducted from the height given by observation at the corresponding hours during the month; if the height of the barometer, at any hour of observation, was less than the mean height at that hour, the difference was put down with the sign −; if greater, with the sign +. By this process the diurnal variation was eliminated. The residual quantities were then arranged in Tables, and the observation corresponding the nearest in time to the moon's culmination being marked for each day, the whole were again re-arranged in lunar tables as follows:—

The moon's superior culmination was assumed as 0 hour of lunar time, and the differences corresponding to that hour placed in the first column; the remaining columns were similarly formed.

The means of the sums of these differences are exhibited in Table I., which consists of two parts, the first part containing the barometric differences at the lunar hours from the superior to the inferior passage, and the second part, from the inferior to the superior passage; the means for each period of six months are shown in the final column of the second part of the Table.

Table II. shows the differences between the mean results and the several numbers in Table I. It must be borne in mind, that had the sets of weekly observations been complete, the sum of the minus differences would have equalled the plus ones: this, however, is not the case owing to the omissions above noted, and an inequality is thereby produced which occasioned the formation of Table I., in which the range of the mean values is shown, the lowest number being assumed as zero.

The means of the complete years 1841, 1844 and 1845, are shown in Table III.; and as two-hourly observations were taken during the first six months of 1842 and of 1843*, and hourly observations during the latter portions of those years, the results of the first six months of 1842 have been combined with the first six months of 1843, and the hourly observations of the latter halves of these years have been combined for the same reason.

In Table IV. the whole of the two-hourly observations, for a period of twenty-four months, have been added together for a general mean, and similarly the whole of the hourly observations for a period of thirty-six months. The results are exhibited in Plate X., which is drawn to ·001 of barometric pressure to 0·74 of an inch of scale.

Finally, Table V. exhibits the observations of three years combined, so as to show the effect upon the barometer of the moon when similarly situated both in its superior and inferior passage; and in a column in juxtaposition is placed a similar table derived from two years' observation at St. Helena, extracted from Colonel Sabine's valuable paper on the subject; from which it will be observed that the effect produced by the moon upon the barometer at Singapore, in the vicinity of the Equator, is slightly greater than at St. Helena, more distant by 14½ degrees of latitude.

* In 1843, in consequence of a deficiency in the number of assistants.

With respect to the oceanic tide, the difference between rise and fall at spring tides varies from 9 to 12 feet. The establishment of the port is about $10^h\ 50^m$ A.M.

Table I.

Mean Barometrical Differences at the several Lunar Hours.

1st Part. From the Superior to the Inferior Passage.

	Mean of each of the Lunar Hours.											
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
First part of 1841	·00603	.....	·00540	.....	·00107	.....	·00077	.....	·00176	.....	·00332	
Second part of 1841	·00619	.....	·00442	.....	·00293	.....	·00184	.....	·00369	.....	·00445	
First part of 1842	·00599	.....	·00470	.....	·00214	.....	·00275	.....	·00000	.....	·00264	
Second part of 1842	·00796	·00685	·00568	·00473	·00315	·00085	·00061	·00000	·00109	·00326	·00355	·00625
First part of 1843	·00372	.....	·00411	.....	·00191	.....	·00291	.....	·00311	.....	·00369	
Second part of 1843	·00728	·00612	·00502	·00391	·00365	·00135	·00000	·00129	·00138	·00324	·00199	·00278
First part of 1844	·00551	·00533	·00405	·00376	·00304	·00050	·00000	·00030	·00141	·00365	·00500	·00739
Second part of 1844	·00882	·00827	·00702	·00541	·00390	·00173	·00004	·00064	·00128	·00215	·00279	·00351
First part of 1845	·00674	·00440	·00353	·00150	·00113	·00000	·00100	·00025	·00114	·00277	·00305	·00422
Second part of 1845	·00774	·00829	·00611	·00571	·00280	·00199	·00024	·00015	·00161	·00341	·00411	·00526

2nd Part. From the Inferior to the Superior Passage.

	Mean of each of the Lunar Hours.												Mean in the six months.
	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	
First part of 1841	·00468	.....	·00500	.....	·00559	.....	·00069	.....	·00060	.....	·00397	.....	·00294
Second part of 1841	·00725	.....	·00594	.....	·00113	.....	·00000	.....	·00097	.....	·00405	.....	·00358
First part of 1842	·00431	.....	·00552	.....	·00286	.....	·00178	.....	·00092	.....	·00490	.....	·00391
Second part of 1842	·00807	·00734	·00629	·00622	·00499	·00409	·00325	·00253	·00313	·00456	·00567	·00690	·00446
First part of 1843	·00301	.....	·00505	.....	·00278	.....	·00000	.....	·00052	.....	·00103	.....	·00261
Second part of 1843	·00372	·00913	·00343	·00600	·00443	·00344	·00178	·00134	·00156	·00265	·00179	·00513	·00303
First part of 1844	·00812	·00692	·00640	·00591	·00371	·00200	·00084	·00006	·00186	·00271	·00335	·00371	·00356
Second part of 1844	·00303	·00456	·00348	·00192	·00100	·00088	·00000	·00162	·00322	·00338	·00428	·00615	·00334
First part of 1845	·00553	·00411	·00282	·00286	·00269	·00110	·00086	·00158	·00116	·00283	·00409	·00530	·00267
Second part of 1845	·00641	·00553	·00333	·00200	·00111	·00014	·00000	·00051	·00043	·00259	·00437	·00646	·00335

Table II.

Numerical Values of the excess or defect of the Barometric Differences at the several Lunar Hours.

1st Part. From the Superior to the Inferior Passage.

	Lunar Hours.											
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
First part of 1841	+·0031	.....	+·0025	.....	−·0020	.....	−·0022	.....	−·0012	.....	+·0004	
Second part of 1841	+·0026	.....	+·0008	.....	−·0006	.....	−·0017	.....	+·0001	.....	+·0010	
First part of 1842	+·0028	.....	+·0015	.....	−·0011	.....	−·0005	.....	−·0032	.....	−·0006	
Second part of 1842	+·0033	+·0024	+·0012	+·0003	−·0013	−·0036	−·0038	−·0045	−·0034	−·0012	−·0009	+·0018
First part of 1843	+·0011	.....	+·0015	.....	−·0007	.....	−·0006	.....	+·0005	.....	+·0011	
Second part of 1843	+·0040	+·0029	+·0018	+·0007	+·0004	−·0019	−·0032	−·0019	−·0018	−·0010	−·0012	−·0004
First part of 1844	+·0019	+·0018	+·0005	+·0002	−·0005	−·0031	−·0036	−·0033	−·0021	+·0001	+·0020	+·0038
Second part of 1844	+·0055	+·0049	+·0037	+·0031	+·0006	−·0016	−·0033	−·0027	−·0021	−·0012	−·0006	+·0002
First part of 1845	+·0041	+·0017	−·0002	−·0012	−·0015	−·0027	−·0011	−·0024	−·0015	+·0001	+·0004	+·0016
Second part of 1845	+·0044	+·0049	+·0028	+·0024	−·0006	−·0014	−·0031	−·0032	−·0017	+·0001	+·0008	+·0019

2nd Part. From the Inferior to the Superior Passage.

	Lunar Hours.											
	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.
First part of 1841	+·0017	.....	+·0021	.....	+·0003	.....	−·0022	.....	−·0029	.....	+·0010	
Second part of 1841 ...	+·0036	.....	+·0024	.....	−·0024	.....	−·0036	.....	−·0026	.....	+·0005	
First part of 1842	+·0011	.....	+·0023	.....	−·0003	.....	−·0014	.....	−·0023	.....	+·0017	
Second part of 1842 ...	+·0036	+·0029	+·0018	+·0018	+·0005	−·0004	−·0012	−·0019	−·0013	+·0001	+·0012	+·0024
First part of 1843	+·0004	.....	+·0025	.....	−·0002	.....	−·0026	.....	−·0021	.....	−·0013	
Second part of 1843 ...	+·0005	+·0009	+·0002	+·0028	+·0012	+·0002	−·0014	−·0019	−·0017	−·0006	−·0014	+·0019
First part of 1844	+·0046	+·0034	+·0028	+·0023	+·0001	−·0016	−·0027	−·0035	−·0017	−·0008	−·0002	+·0002
Second part of 1844 ...	−·0003	+·0012	+·0001	−·0014	−·0023	−·0025	−·0033	−·0017	−·0001	·0000	+·0009	+·0028
First part of 1845	+·0029	+·0014	+·0002	+·0002	·0000	−·0016	−·0018	−·0011	−·0015	+·0002	+·0013	+·0026
Second part of 1845 ...	+·0031	+·0022	·0000	−·0013	−·0022	−·0039	−·0033	−·0028	−·0029	−·0008	+·0010	+·0031

Table III.

Excess or Defect of the Barometric Differences at the several Lunar Hours shown in Annual Means.

1st Part. From the Superior to the Inferior Passage.

	Lunar Hours.											
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1841	+·0028	.....	+·0016	.....	−·0013	.....	−·0020	.....	−·0005	.....	+·0007	
First part of 1842, and first part of 1843 ...	+·0020	.....	+·0015	.....	−·0009	.....	−·0006	.....	−·0013	.....	+·0003	
Second part of 1842, and second part of 1843...	+·0036	+·0026	+·0015	+·0005	−·0008	−·0027	−·0035	−·0032	−·0026	−·0011	−·0010	+·0007
1844	+·0037	+·0032	+·0021	+·0016	·0000	−·0024	−·0034	−·0030	−·0021	−·0005	+·0007	+·0020
1845	+·0042	+·0033	+·0014	+·0006	−·0010	−·0020	−·0021	−·0028	−·0016	+·0001	+·0006	+·0018

2nd Part. From the Inferior to the Superior Passage.

	Lunar Hours.											
	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.
1841	+·0026	.....	+·0022	.....	−·0010	.....	−·0029	.....	−·0027	.....	+·0007	
First part of 1842, and first part of 1843 ...	+·0008	.....	+·0024	.....	−·0003	.....	−·0024	.....	−·0022	.....	+·0002	
Second part of 1842, and second part of 1843...	+·0020	+·0019	+·0010	+·0023	+·0008	−·0001	−·0013	−·0019	−·0015	−·0003	−·0001	+·0002
1844	+·0021	+·0023	+·0013	+·0004	−·0011	−·0020	−·0030	−·0026	−·0009	−·0004	+·0004	+·0015
1845	+·0030	+·0018	+·0001	−·0005	−·0011	−·0024	−·0026	−·0020	−·0022	−·0003	+·0019	+·0029

Table IV.

Excess or Defect of the Barometric Differences at the several Lunar Hours as severally deduced from Hourly and 2-Hourly Observations.

1st Part. From the Superior to the Inferior Passage.

	Lunar Hours.											
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Mean of two years.....	+·0024	.....	+·0015	.....	−·0011	.....	−·0013	.....	−·0009	.....	+·0005	.....
Mean of three years	+·0033	+·0030	+·0017	+·0009	−·0006	−·0024	−·0030	−·0030	−·0021	−·0005	+·0001	+·0015

2nd Part. From the Inferior to the Superior Passage.

	Lunar Hours.											
	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.
Mean of two years.....	+·0017	.....	+·0023	.....	−·0007	.....	−·0025	.....	−·0025	.....	+·0005	
Mean of three years	+·0024	+·0020	+·0008	+·0007	−·0005	−·0015	−·0023	−·0022	−·0015	−·0003	+·0004	+·0022

Table V.

Mean Variation of Barometric Pressure deduced from the Hourly Observations.

Moon's distance from the meridian.	Variations of barometric pressure.				Hourly variations.		Mean of three years at Singapore.	Mean of two years at St. Helena.	Moon's distance from the meridian.
	At the hours following the meridian passage.		At the hours preceding the meridian passage.		From the observations at the hours following the meridian passage.	From the observations at the hours preceding the meridian passage.			
h	h in.	in.	h in.	in.	in.	in.	in.	in.	h
0	0 +·0038 12 +·0024	+·0031	0 +·0038 12 +·0024	+·0031	+·0057	+·0057	+·00570	+·00365	0
1	1 +·0030 13 +·0020	+·0025	11 +·0015 23 +·0022	+·0018	+·0051	+·0044	+·00475	+·00336	1
2	2 +·0017 14 +·0008	+·0012	10 +·0001 22 +·0004	+·0002	+·0038	+·0028	+·00330	+·00275	2
3	3 +·0009 15 +·0007	+·0008	9 −·0005 21 −·0003	−·0004	+·0034	+·0022	+·00280	+·00158	3
4	4 −·0006 16 −·0005	−·0005	8 −·0021 20 −·0015	−·0018	+·0021	+·0008	+·00145	+·00110	4
5	5 −·0024 17 −·0015	−·0019	7 −·0030 19 −·0022	−·0026	+·0007	·0000	+·00035	+·00046	5
6	6 −·0030 18 −·0023	−·0026	6 −·0030 18 −·0023	−·0026	·0000	·0000	·00000	·00000	6

X. *Discovery that the Veins of the Bat's Wing (which are furnished with valves) are endowed with rythmical contractility, and that the onward flow of blood is accelerated by each contraction. By* T. Wharton Jones, *F.R.S., Fullerian Professor of Physiology in the Royal Institution of Great Britain, Ophthalmic Surgeon to University College Hospital, and Corresponding Member of the Society of Biology of Paris, &c. &c.*

Received November 20, 1851.—Read February 5, 1852.

In entering on the investigation of the state of the blood and the blood-vessels in inflammation excited in the web of the Bat's wing, I applied myself, in the first place, to the study of the distribution, structure and endowments of the arteries, capillaries and veins of the part, and of the phenomena of the circulation in them.

I had not observed the circulation under the microscope long, before I was struck with something peculiar in the flow of blood in the veins; I therefore directed my attention to them, and discovered that they contracted and dilated rythmically. Following the veins for some extent in their course, I further discovered them to be provided with valves, some of which completely opposed regurgitation of blood, others only partially.

The cause of the peculiarity in the flow of blood in the veins was thus no longer doubtful, but some continued observation was required before I was able to make out exactly its mode of operation.

The act of contraction of the vein is manifested by progressive constriction of its caliber and increasing thickness of its wall; the relaxation of the vessel, by a return to the former width of caliber and thickness of wall.

The rythmical contractions and dilatations of the veins are, in the natural state, continually going on; but sometimes with greater, sometimes with less rapidity, and sometimes to a greater, sometimes to a less extent. The average number of contractions in a minute, I have found to be ten. I have on some occasions counted only seven or eight, and on other occasions as many as twelve or thirteen. Most usually, the numbers were nine and eleven. The supervening dilatations take place rather more quickly than the contractions. The amount of constriction of one of the larger veins—one about $\frac{1}{300}$dth or $\frac{1}{400}$dth of an inch in width when dilated—at each contraction of its walls, may be put down at a fourth or fifth of its whole width when in a state of dilatation; I have sometimes estimated it at nearly a third, sometimes at not more than a sixth.

The contractions *centrad* and *distad* of a valve appeared to be simultaneous, as did also the dilatations.

The smaller veins, those of the first and second order, proceeding from the radicles, contract, but not in a very marked manner, and are destitute of valves.

During contraction, the flow of blood in the vein is accelerated. On the cessation of the contraction, the flow is checked, and a tendency to regurgitation of the blood takes place, which brings the valves into play. Where the valves are perfect, the backward movement of the blood is at once stopped by their closure; but where the valves are not complete, the blood regurgitates more or less freely*. But this check to the onward flow of the blood is usually only for a moment or two. Already, even while the vein is in the act of again becoming dilated, the onward flow of blood recommences and goes on, though comparatively slowly, until dilatation is completed and contraction supervenes; whereupon acceleration of the flow takes place as before.

It is to be observed, that in determining the flow of blood in the veins (the phenomena of which I have now described), the action of the heart is concerned as well as the contractions of the veins themselves. It appears to be the heart's action which maintains the onward flow of blood during the dilatation of the vein, whilst it is the contraction of the vein, coming in aid of the heart's action, which causes the acceleration. Sometimes the *vis a tergo* is sufficient to keep up a pretty steady flow in the veins, this being only accelerated at each contraction of these vessels.

The check to the flow of blood in the veins takes place at the completion of the contraction or commencement of the dilatation. The number of checks observable in a minute, therefore, corresponds with the number of contractions. In one case, while an assistant marked the time by a seconds' watch, I observed that a complete valve checked the tendency to regurgitation nine times in a minute; and on counting the number of contractions of the same vessel, I found them also nine in a minute. In another case, eleven checks and eleven contractions were counted; and so on repeatedly. Though I quote these little experiments, I would remark that, after some practice in the observation, the eye is quite able to take in at one glance the succession and relations of the two phenomena.

The valves of the veins are composed sometimes of but a single flap, sometimes of two. In the situation of a valve, and *centrad* of the insertion of its flaps, the veins present the usual dilatations or sinuses corresponding to the sinuses of Valsalva at the origin of the pulmonary artery and aorta. These sinuses are best seen when the valve happens to present its flaps edgeways to the observer.

Valves are found close to the entrance of a large branch, but distad of it (Plate IV. fig. 2). They are also found at intermediate parts of the veins (fig. 1). Tracing the

* Sometimes, as for example, into a venous branch with an incomplete valve, a retrograde flow of blood takes place from a large vein, at the moment this latter is contracting and propelling its blood onwards.—May 7, 1852.

veins from radicles to trunks, the first valves I have noticed were at the junction of the second order of veins to form the third.

In watching the circulation, it is interesting to observe the backward eddy of blood-corpuscles into the sinuses of the valves, when the blood issues from the narrow valvular opening into the wide part of the vein beyond (fig. 1).

In structure, the valves are seen to be a reduplication of the clear innermost coat of the vein, with sometimes a pretty evident layer of fibrous tissue intervening.

Each vein is closely accompanied by an artery, a nerve only intervening. The average diameter of a vein is to that of its accompanying artery as about 3 to 2.

The contractility of the arteries is altogether different in its nature from that of the veins. It is *tonic contractility, not rythmical.* On the application of pressure over an artery, this vessel may be seen to become constricted, sometimes even to temporary obliteration of its caliber, and that uniformly throughout some extent of its course, both above and below the point where the pressure was applied; or, the constriction is greater or less at intervals, so that the vessel presents a varicose appearance. This tonic contraction of the arteries of the Bat's wing does not take place quite so quickly as the same phenomenon in the Frog's web, and, ordinarily, continues a longer time*.

The pulsation of a vein so affects its accompanying artery as to push the latter, as a whole, to and fro. That the movement of the artery referred to is really owing to this cause, and not to any pulsation or rythmical contraction and dilatation of its own walls, is evident from this, that the movements are synchronous with the contractions and dilatations of the vein, and that *both sides of the artery move in the same direction*, not approximating and receding from each other, so as to constrict or dilate the caliber, as in the case of the vein.

I have not been able to observe unequivocal evidences of tonic contractility of veins in addition to their rythmical contractility. When pressure is, at the same time, applied over the vein as well as the artery, the vein is not found to become tonically constricted in the same manner as the artery, upward and downward. At the place where the vein was pressed on, a mechanical indentation of its wall may perhaps be seen. And in addition to this, there may often be observed an appearance of great and abrupt constriction. This appearance, however, is not owing to contraction of the walls of the vein, but to a deposit of a viscid-looking grayish granular lymph within the vessel at the place, obstructing its channel and narrowing the stream of blood (figs. 3 and 4). On watching, I have seen portions of this deposit detached and carried away by the stream of blood, with corresponding enlargement of the channel, and again an additional deposit with renewed narrowing of the stream. When the pressure has been considerable, I have seen the vein become for a time wholly obstructed by the deposit. A similar deposit of lymph takes place in the artery. In one case, I observed that the artery, at the place pressed on, was

* When a frog under examination struggles, the arteries of the web are seen to become constricted. I have observed the same thing in the web of the Bat's wing and the ear of a white rabbit.

actually not so much constricted as above and below, though, on account of the narrowness of the stream of blood from the presence of the lymphy deposit, it appeared as much so at first sight (fig. 4).

Having subjected the web to the galvanic influence from a single pair of plates, I found all the smaller arteries of the part in a state of considerable tonic constriction, but the larger arteries constricted in a less degree. The effect of galvanism on the veins appeared to be to render their rythmical contractions somewhat more brisk, they having been previously rather languid. On cutting a vein across, I did not observe tonic constriction of it, any more than in the Frog.

After the application of a drop of Vinum opii to the web, the veins were found dilated as well as the arteries, and their rythmical contractions appeared to be suspended.

It has been stated by an authority not liable to err, that, on mechanical irritation, both artery and vein of the Bat's web gradually contract and close, and, by and by, dilate wider than before. And, again, that in Bats, contraction of veins is quite as well marked as that of arteries.

These statements, it will be observed, imply tonic contractility of the veins.

Notwithstanding my attention has been repeatedly directed to the point, I have not, as previously stated, been able to observe unequivocal evidences of tonic contractility of veins, in addition to their rythmical contractility. For this reason, I cannot help venturing on the supposition that Mr. Paget* must have made his statements either from a hasty and imperfect observation of the proper rythmical contractions of the veins; or, seeing that in rythmical contraction of the veins the constriction is never to closure, like that of the arteries, under some such misapprehension as to the nature of the vessel observed, as he certainly must have laboured under when he supposed that arteries and veins of the second and third order open directly into each other without any intermedium of capillaries.

The arteries and their subdivisions anastomose freely with each other, forming a network all through the web, the meshes of which go on to diminish towards the free margin. Each artery and each subdivision of an artery is closely accompanied by a vein; and these veins, like the arteries they accompany, anastomose with each other. But it is to be remarked that nowhere do the arteries and veins directly communicate. The only communication is the usual one through the medium of capillaries. The capillaries, the walls of which are destitute of contractility, receive the blood from small arterial twigs which arise from the arterial network, and return it to the venous radicles which open into corresponding veins. These arterial twigs, capillaries and venous radicles, form networks within the meshes of the great vascular network, and a looped network at the margin of the web† (Plate V.).

The observations recorded in the preceding pages were made principally with

* Lectures on Inflammation at the Royal College of Surgeons in 1850.

† I shall have occasion to treat of this point more in detail in a paper on the state of the blood and the blood-vessels in inflammation of the web of the Bat's wing.

one-eighth of an inch object-glass, and the two lowest eyepieces, affording magnifying powers of 370 and 550 diameters.

The web of the wing was stretched out on the object plate, wetted on both sides with water, and covered with a thin plate of glass at the spot to be examined.

Appendix to the Foregoing Paper.

Received December 11, 1851,—Read February 5, 1852.

In consequence of the dark pigment in the cells of the epidermis of the web of the Bat's wing, the structure of the vessels cannot be well made out except by dissection.

A small piece of the web containing vessels being detached and disposed in a drop of water, under the simple microscope, the two layers of skin may be readily torn from each other with needles, and the artery and vein with their accompanying nerve, which lies between the two, separated in one bundle.

In pieces cut out of a web which had been dried, the bundle of vessels and nerve was, after tearing away the skin, left surrounded by a sheath of cellular and elastic fibres disposed longitudinally; but in pieces cut out from the living web and directly examined, this sheath was always detached along with the skin, and the vessels with their accompanying nerve at once laid bare (see Plate IV. fig. 5.).

Both artery and vein are seen to have a middle coat of circularly disposed muscular fibres; but the appearance of the fibres is different in the two vessels.

The fibres of the vein are about $\frac{1}{3600}$dths of an inch broad, pale, grayish, semitransparent and granular-looking. In general aspect they very much resemble the muscular fibres of the lymphatic hearts of the frog. In none of the muscular fibres of the vein, however, did I detect an unequivocal appearance of transverse marking.

The fibres of the middle coat of the artery are not so pale-looking as those of the middle coat of the vein, are clearer, and exhibit a more strongly marked contour.

Second Appendix.

Received May 10,—Read May 13, 1852.

From a microscopical examination of the blood-vessels and circulation in the ears of the Long-eared Bat, I have ascertained that, different from what I discovered to be the case in the wings, the veins of the ears are unfurnished with valves, and are not endowed with rythmical contractility, and that the onward flow of blood in them is consequently uniform. I ought, perhaps, to qualify the statement that the veins of the ear are not endowed with rythmical contractility, by saying, that I think I noticed a very slight tendency to it, here and there in a vein, but so slight as not to have the smallest effect on the flow of blood.

This observation regarding the ear of the Bat illustrates how that the heart's action is sufficient of itself for the circulation of the blood in the body generally; but that being sufficient for that only, the supplementary force of rythmical con-

tractility of veins, supported by the presence of valves, is called forth to promote the flow of blood in the wings, which, on account of their extent, are, as regards their circulation, in a considerable degree, though not entirely, beyond the sphere of the heart's influence.

I may take this opportunity to mention that I have also found the veins of the mesentery of the Mouse destitute of rythmical contractility.

Explanation of the Plates.

PLATE IV.

Fig. 1. A vein with a complete valve. In this figure an attempt has been made to represent the backward eddy of blood-corpuscles into the sinuses of the valve, at the time the blood issues from the narrow valvular opening into the wide part of the vein beyond.

Fig. 2. A vein with a valve close to the entrance of a large branch.

Fig. 3. An artery and vein over which pressure had been applied. The artery is seen constricted at intervals, above and below the place of pressure *a*. The vein is not constricted, but there is seen, at the place where the pressure was applied, a grayish granular deposit of lymph within the vessel, giving rise to the appearance of constriction by narrowing the stream of blood.

Fig. 4. Representation of the case in which the artery, at the place pressed on, was actually not so much constricted as above and below; though, on account of the narrowness of the stream of blood from the presence of the lymphy deposit, it appeared as much so at first sight.

In this case the channel of the vein was much narrowed by a deposit of lymphy matter on either side within the vessel.

Fig. 5. This represents an artery (*a*) and a vein (*v*), with an accompanying nerve (*n*) lying between the two, as seen with a magnifying power of about 370 diameters; immediately after being separated, by dissection under the simple microscope, from a small piece cut out of the living web. The cellular sheath was detached along with the two layers of skin.

The artery is at one place tonically constricted, and there the middle coat is seen to be thicker.

The difference in the general aspect of the fibres of the middle coats of the two vessels may be recognised.

PLATE V.

This represents a portion of the vascular network of the web of the Bat's wing, as seen under a low magnifying power. The arteries are observed to anastomose with each other, and the veins with each other; but nowhere are the arteries and veins seen to communicate directly. The only communication, it is to be observed, is through the medium of the capillaries.

XI. *Experimental Researches in Electricity.—Twenty-ninth Series.* By MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Ord. Boruss. Pour le Mérite, Eq., Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, Munich, Bruxelles, Vienna, Bologna, &c. &c.*

Received December 31, 1851,—Read March 25 and April 1, 1852.

§ 35. *On the employment of the Induced Magneto-electric Current as a test and measure of Magnetic Forces.*

3177. THE proposition which I have made to use the induced magneto-electric current as an experimental indication of the presence, direction and amount of magnetic forces (3074.), makes it requisite that I should also clearly demonstrate the principles and develope the practice necessary for such a purpose; and especially that I should prove that the amount of current induced is precisely proportionate to the amount of lines of magnetic force intersected by the moving wire, in which the electric current is generated and appears (3082, 3109.). The proof already given is, I think, sufficient for those who may repeat the experiments; but in order to accumulate evidence, as is indeed but proper in the first announcement of such a proposition, I proceeded to experiment with the magnetic power of the earth, which presents us with a field of action, not rapidly varying in force with the distance, as in the case of small magnets, but one which for a given place may be considered as uniform in power and direction; for if a room be cleared of all common magnets, then the terrestrial lines of magnetic force which pass through it, have one common direction, being that of the dip, as indicated by a free needle or other means, and are in every part in equal proportion or quantity, *i.e.* have equal power. Now the force being the same everywhere, the proportion of it to the current evolved in the moving wire is then perhaps more simply and directly determined, than in the case where, a small magnet being employed, the force rapidly changes in amount with the distance.

¶ i. *Galvanometer.*

3178. For such experimental results as I now propose to give, I must refer to the galvanometer employed and the precautions requisite for its proper use. The instrument has been already described in principle (3123.), and a figure of the conductor which surrounds the needles, given. This conductor may be considered as a square copper bar, 0·2 of an inch in thickness, which passes twice round the plane of vibration of each of the needles forming the astatic combination, and then is continued outwards and terminates in two descending portions, which are intended to dip into

cups of mercury. As both the needles are within the convolutions of this bar, an indicating bristle or fine wire of copper is fixed parallel to, and above them upon the same axis, and this, in travelling over the usual graduated circle, shows the place and the extent of vibration or swing of the needles below. The suspension is by cocoon silk, and in other respects the instrument is like a good ordinary galvanometer.

3179. It is highly important that the bar of copper about the needles should be perfectly clean. The vertical zero plane should, according to the construction, be midway between the two vertical coils of the bar, fig. 1; instead of which the needle at first pointed to the one side or the other, being evidently attracted by the upright portions of the bar. I at first feared that the copper was magnetic, but on cleaning the surface carefully with fine sand-paper, I was able to remove this effect, due no doubt to iron communicated by handling or the use of tools, and the needle then stood truly in a plane equidistant from the two coils, when that plane corresponded with the magnetic meridian.

Fig. 1.

3180. The connexions for this galvanometer (3123, 3133.) were all of copper rod or wire 0·2 of an inch in diameter; but even with wires of this thickness the extent of the conductors should not be made more than is necessary; for the increase from 6 to 8, 10 or 12 feet in length, makes a considerable difference at the galvanometer, when electric currents, low in intensity, are to be measured. It is most beautiful to observe in such cases the application of Ohm's law of currents to the effects produced. When the connexions were extended to a distance, straight lengths of wire with dropping ends were provided, and these by dipping into cups of mercury completed the connexion and circuit. The cups consisted of cavities turned in flat pieces of wood. The ends of the connecting rods and of the galvanometer bar were first tinned, and then amalgamated; after which their contact with the mercury was both ready and certain. Even where connexion had to be made by contact of the solid substances, I found it very convenient and certain to tin and amalgamate the ends of the conductors, wiping off the excess of mercury. The surfaces thus prepared are always ready for a good and perfect contact.

3181. When the needle has taken up its position under the earth's influence, and the copper coil is adjusted to it, the needle ought to stand at true zero, and appears so to do. When that is really the case, equal forces applied in succession on opposite sides of the needle (by two contrary currents through the coil for instance) ought to deflect the needle *equally* on both sides, and they do so. But sometimes, when the needle appears to stand at zero, it may not be truly in the magnetic meridian; for a little torsion in the suspension thread, even though it be only 10° or 15° (for an indifferent needle), and quite insensible to the eye looking at the magnetic needle, does deflect it, and then the force which opposes the swing of the needle, and which stops and returns the needle towards zero (being due both to the torsion and the earth's force), is not equal on the two sides, and the consequence is, that the extent of

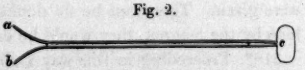
swing in the two directions is not equal for equal powers, but is greater on one side than the other.

3182. I have not yet seen a galvanometer which has an adjustment for the torsion of the suspending filament. Also, there may be other causes, as the presence about a room, in its walls and other places, of unknown masses of iron, which may render the forces on opposite sides of the instrument zero unequal in a slight degree; for these reasons it is better to make *double observations*. All the phenomena we have to deal with, present effects in two contrary directions. If a loop pass over the pole of a magnet (3133.), it produces a swing in one direction; if it be taken away, the swing is in the other direction; if the rectangles and rings to be described (3192.) be rotated one way, they produce one current; if the contrary way, the other and contrary current is produced. I have therefore always in measuring the power of a pole or the effect of a revolving intersecting wire made many observations in both directions, either alternately or irregularly; have then ascertained the average of those on the one side, and also on the other (which have differed in different cases from $\frac{1}{50}$th to $\frac{1}{300}$th part), and have then taken the mean of these averages as the expression of the power of the induced electric current, or of the magnetic forces inducing it.

3183. Care must be taken as to the position of the instrument and apparatus connected with it, in relation to a fire or sources of different temperatures, that parts which can generate thermo-currents may not become warmed or cooled in different degrees. The instrument is exceedingly sensible to thermo-electric currents; the accidental falling of a sun-beam upon one of two connecting mercury cups for a few moments disturbed the indications and rendered them useless for some time.

3184. In order to ascertain practically, *i. e.* experimentally, the comparative value of degrees in different parts of the scale or graduation of this instrument and so to render it a measurer, the following trials were made. A loop like that before described (3133.), fig. 2, was connected with the galvanometer by communications which removed the loop 9 feet from the instrument, and it was then fixed. A compound bar-magnet consisting of two plates, each 12 inches long, 1 inch broad, and 0·5 in thickness, was selected of such strength as to lift a bunch of clean iron filings, averaging 45 grains at either extremity. Blocks were arranged at the loop, so that this magnet, held in a vertical position, could have one end passed downwards through the loop until the latter coincided with the equator of the magnet (3191.); after which it could be quickly removed and the same operation be repeated at pleasure. When the magnet was thus moved, the loop being unconnected (at one of the mercury cups) with the galvanometer, there was no sensible change of place in the needles; the direct influence of the magnet at this distance of 9 feet being too small for such an effect.

Fig. 2.



3185. It must be well understood, that, in all the observations made with this instrument, the *swing* is observed and counted as the effect produced, unless otherwise expressed. A constant current in an instrument will give a constant and continued

deflection, but such is not the case here. The currents observed are for short periods, and they give, as it were, a blow or push to the needle, the effect of which, in swinging the needle, continues to increase the extent of the deflection long after the current is over. Nevertheless the extent of the swing is dependent on the electricity which passed in that brief current; and, as the experiments seem to indicate, is simply proportional to it, whether the electricity pass in a longer or a shorter time (3104.), and notwithstanding the comparative variability of the current in strength during the time of its continuance.

3186. The compound bar being introduced once into the loop and left there, the swing at the galvanometer was observed and found to be 16°; the galvanometer needle was then brought to zero, and the bar removed, which gave a reverse current and swing, and this also was 16°. Many alternations, as before described, gave 16° as the mean result, *i. e.* the result of one intersection of the lines of force of this magnet (3102.). In order to comprehend the manner in which the effect of two or more intersections of these lines of force were added together, it should be remembered that a swing of the needle from right to left occupied some time (13 seconds); so that one is able to introduce the magnet into the loop, then break the electric circuit by raising one end of the communicating wire out of the mercury, remove the magnet, which by this motion does nothing, restore the mercury contact, and reintroduce the magnet into the loop, before a tenth part of the time has passed, during which the needles, urged by the first impulse, would swing. In this way two impulses could be added together, and their joint effect on the needle observed; and, indeed, by practice, three and even four impulses could be given within the needful time, *i. e.* within one-half or two-thirds of the time of the full swing; but of course the latter impulses would have less power upon the needles, because these would be more or less oblique to the current in the copper coil at the time when the impulses were given. There can be no doubt, that, as regarded the currents induced in the loop by the magnet, they would be equal on every introduction of the same magnet.

3187. Proceeding in this way I obtained results for one, two, three, and even four introductions with the same magnet.

One introduction	15°
Two introductions	31·25
Three introductions	46·87
Four introductions	58·50

Here the approximation to 1, 2, 3, 4 cannot escape observation*; and I may remark,

* See note to (3189.) $\sin\frac{15}{2} = \sin 7^\circ 30' = \cdot 130526$ $\quad \cdot 130526$

$\sin\frac{31\cdot25}{2} = \sin 15\cdot625 = \sin 15\ 37\cdot5 = \cdot 269200$ $\quad \frac{269200}{2} = \cdot 134600$

$\sin\frac{46\cdot87}{2} = \sin 23\cdot435 = \sin 23\ 26\cdot1 = \cdot 3976818$ $\quad \frac{3976818}{3} = \cdot 1328606$

$\sin\frac{58\cdot50}{2} = \sin 29\cdot25 = \sin 27\ 15 = \cdot 4886212$ $\quad \frac{4886212}{4} = \cdot 1221553$

that, whilst observing the place attained at the end of a swing which is retained only for an instant, some degree of error must creep in; and that that error must be greatest, in the first number, where it falls altogether upon the unit of comparison than in the other observations, where only one-half or one-third of it is added to a half or a third of the whole result. Thus, if we halve the arc for two introductions of the pole, it gives 15°·625; if we take the third of that for three introductions, it gives 15°·61;—numbers which are almost identical, so that if the first number was increased by only 0°·6, the proportion would be as 1, 2 and 3. The reason why the fourth, which is 14°·625, is less, may perhaps be referred to the cause already assigned, namely, the declination distance of the needle from the coil when that impulse was given (3186.).

3188. In order to avoid in some degree this case, and to compare the degrees at the beginning of the scale, which are most important for the comparison of future experiments with one another, I took one of the bars of the compound magnet employed above (3184.). The results were as follows:—

One introduction	8°
Two introductions	15·75
Three introductions	23·87
Four introductions.	31·66

which numbers are very closely as 1, 2, 3 and 4. If we divide as before, we have 8°, 7°·87, 7°·95, 7°·91; so that if only 0·09 be subtracted from the first observation, or 8°, it leaves that simple result*.

3189. Hence it appears, that in this mode of applying and measuring the magnetic powers, the number of degrees of swing deflection are for small arcs nearly proportional to the magnetic force which has been brought into action on the moving wire†.

* $\sin\frac{8}{2}=\sin 4^\circ = \cdot0697565$ $\qquad \cdot0697565$

$\sin\frac{15\cdot75}{2}=\sin 7\cdot875=\sin 7^\circ 52'5=\cdot1370123$ $\qquad \frac{1370123}{2}=\cdot0685061$

$\sin\frac{23\cdot87}{2}=\sin 11\cdot935=\sin 11\ 56'1=\cdot2068019$ $\qquad \frac{2068019}{3}=\cdot0689340$

$\sin\frac{31\cdot66}{2}=\sin 15\cdot83=\sin 15\ 49'8=\cdot2727840$ $\qquad \frac{2727840}{4}=\cdot0681960$

† Mr. Christie has recalled my attention to a paper in the Philosophical Transactions, 1833, p. 95, in which he has investigated, at p. 111, &c., the effect of what may be called magneto-electric impulses in deflecting the magnetic needle. He found that the velocity of the projection of the needle, which is a measure of the force acting upon it at the instant of its moving, will be proportional to the sine of half the arc of swing. My statement, therefore, would as a general expression be erroneous; but for small arcs the results as given by it are not far from the truth. The error does not interfere with the general reasoning and conclusions of the paper; and as the numbers are the results of experiment, which, though made with a first and therefore rough apparatus, were still made with some care, and are expressed simply as deflections, I prefer their appearance as they are rather than in an altered state. Mr. Christie has been so kind as to give me the true expression of force for many of the cases, and I have inserted the results as foot-notes where the cases occur.—Jan. 26, 1852.

3190. I have found the needles very constant in their strength for days and weeks together. By care, the constancy of their state for a day is easily secured, and that is all that is required in comparative experiments. Those which I have in use weigh with their axis and indicating wire 9 grains; and when out of the copper coil vibrate to and fro once in 26 seconds.

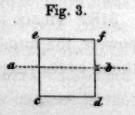
3191. With this instrument thus examined, I repeated most of the experiments with loops formerly described (3133. &c.), with the same results as before. It was also ascertained that the equator of a regular bar-magnet was the place at which the loop should be arrested, to produce the maximum action; and that if it came short of, or passed beyond that place, the final result was less. Employing a magnet 12 inches long, when the loop passed

2·3 inches over the pole the deflection was	5°·91
4·1 inches over the pole the deflection was	7·50
5·1 inches over the pole the deflection was	7·74
6·1 inches over the pole the deflection was	8·16
8·0 inches over the pole the deflection was	7·75
9·0 inches over the pole the deflection was	6·50

¶ ii. *Revolving Rectangles and Rings**.

3192. The form of moving wire which I have adopted for experiments with the magnetic forces of the earth (3177.), is either that of a rectangle or a ring. If a wire rectangle (fig. 3) be placed in a plane, perpendicular to the dip and then turned once round the axis *a b*, the two parts *c d* and *e f* will twice intersect the lines of magnetic force within the area *c e d f*. In the first 180° of revolution the contrary direction in which the two parts *c d* and *e f* intersect those lines, will cause them to conspire in producing one current, tending to run round the rectangle (161) in a given direction; in the following 180° of revolution they will combine in their effect to produce a contrary current; so that if the first current is from *d* by *c e* and *f* to *d* again, the second will be from *d* by *f e* and *c* to *d*. If the rectangle, instead of being closed, be open at *b*, and the ends there produced be connected with a commutator, which changes sides when the rectangle comes into the plane perpendicular to the dip, *i. e.* at every half revolution, then these successive currents can be gathered up and sent on to the galvanometer to be measured. The parts *c e* and *d f* of the

Fig. 3.

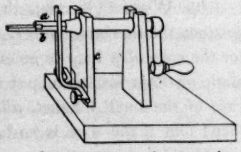


* A friend has pointed out to me that in July 1832, Nobili made experiments with rotating rings or spirals subject to the earth's magnetic influence; they were subsequent to and consequent upon my own experiments upon swinging wires (171, 148.) and revolving globes (160.) of January 1832; but he extended the considerations to the *thickness* of the wire; the *diameter* of the spirals and the *number* of the spirals dependent upon the *length* of the wire. The results (tabulated) will be found in vol. i. page 244, &c. of the Florence edition of his Mémoires.—March 1, 1852.

rectangle may be looked upon simply as conductors; for as they do not in their motion intersect any of the lines of force, so they do not tend to produce any current.

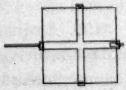
3193. The apparatus which carries these rectangles, and is also the commutator for changing the induced currents, consists of two uprights, fixed on a wooden stand, and carrying above a wooden horizontal axle, one end of which is furnished with a handle, whilst the other projects, and is shaped as in fig. 4. It may there be seen, that two semi-cylindrical plates of copper *a b* are fixed on the axle, forming a cylinder round it, except that they do not touch each other at their edges, which therefore leave two lines of separation on opposite sides of the axle. Two strong copper rods, 0·2 of an inch in diameter, are fixed to the lower part of the upright *c*, terminating there in sockets with screws for the purpose of receiving the ends of the rods proceeding from the galvanometer cups (3180.): in the other direction the rods rise up parallel to each other, and being perfectly straight, press strongly against the curved plates of the commutator on opposite sides: the consequence is, that, whenever in the rotation of the axle, the lines of separation between the commutator plates arrive at and pass the horizontal plane, their contact with these bearing rods is changed, and consequently the direction of the current proceeding from these plates to the rods, and so on to the galvanometer, is changed also. The other or outer ends of the commutator plates are tinned, for the purpose of being connected by soldering to the ends of any rectangle or ring which is to be subjected to experiment.

Fig. 4.



3194. The rectangle itself is tied on to a slight wooden cross (fig. 5), which has a socket on one arm that slides on to and over the part of the wooden axle projecting beyond the commutator plates, so that it shall revolve with the axle. A small copper rod forms a continuation of that part of the frame which occupies the place of axle, and the end of this rod enters into a hole in a separate upright, serving to support and steady the rectangle and its frame. The frames are of two or three sizes, so as to receive rectangles of 12 inches in the side, or even larger, up to 36 inches square. The rectangle is adjusted in its place, so that it shall be in the horizontal plane when the division between the commutator plates is in the same plane, and then its extremities are soldered to the two commutator plates, one to each. It is now evident, that when dealing with the lines of force of the earth, or any other lines, the axle has only to be turned until the upright copper rods touch on each side at the separation of the commutator plates, and then the instrument adjusted in position, so that the plane of the ring or rectangle is perpendicular to the direction of the lines of force which are to be examined, and then any revolution of the commutator and intersecting wire will produce the maximum current which such wire and such magnetic force can produce. The lines of terrestrial magnetic force are inclined at an angle

Fig. 5.



of 69° to the horizontal plane. As, however, only comparative results were required, the instrument was, in all the ensuing experiments, placed in the horizontal plane, with the axis of rotation perpendicular to the plane of the magnetic meridian; under which circumstances no cause of error or variation was introduced into the results. As no extra magnet was employed, the commutator was placed within 3 feet of the galvanometer, so that two pieces of copper wire 3 feet long and 0·2 of an inch in thickness, sufficed to complete the communication. One end of each of these dipped into the galvanometer mercury cups, the other ends were tinned, amalgamated, introduced into the sockets of the commutator rods (3193.), and secured by the pinching screw (fig. 4).

3195. When a given length of wire is to be disposed of in the form best suited to produce the maximum effect, then the circumstances to be considered are contrary for the case of a loop to be employed with a small magnet (39. 3184.), and a rectangle or other formed loop to be employed with the lines of terrestrial force. In the case of the small magnet, *all* the lines of force belonging to it are inclosed by the loop; and if the wire is so long that it can be formed into a loop of two or more convolutions, and yet pass over the pole, then twice or many times the electricity will be evolved that a single loop can produce (36.). In the case of the earth's force, the contrary result is true; for as in circles, squares, similar rectangles, &c. the areas inclosed are as the squares of the periphery, and the lines of force intersected are as the areas, it is much better to arrange a given wire in one simple circuit than in two or more convolutions. Twelve feet of wire in one square intersects in one revolution the lines of force passing through an area of nine square feet, whilst if arranged in a triple circuit, about a square of one foot area, it will only intersect the lines due to that area; and it is thrice as advantageous to intersect the lines within nine square feet once, as it is to intersect those of one square foot three times.

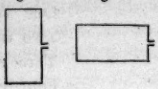
3196. A square was prepared, containing 4 feet in length of copper wire 0·05 of an inch in diameter; it inclosed one square foot of area, and was mounted on the commutator and connected in the manner already described (3194.). Six revolutions of it produced a swing deflection of 14° or 15°, and twelve quick revolutions were possible within the required time (3104.). The results of *quick* and *slow* revolutions were first compared. Six slow revolutions gave as the average of several experiments 15°·5 swing. Six moderate revolutions gave also an average of 15°·5; six quick revolutions gave an average of 15°·66. At another time twelve moderate revolutions gave an average of 28°·75, and twelve quick revolutions gave an average of 31°·33 swing. As before explained (3186.), the probable reason why the quick revolutions gave a larger result than the moderate or slow revolutions is, that in slow time the later revolutions are performed at a period when the needle is so far from parallel with the copper coil of the galvanometer, that the impulses due to them are less effectually exerted. Hence a small or moderate number of revolutions and a quick motion is

best. The difference in the extreme case is less than might have been expected, and shows that there is no practical objection in this respect to the method proposed of experimenting with the lines of magnetic force.

3197. In order to obtain for the present an expression of the power of the earth's magnetic force by this rectangle, observations were made on both sides of zero, as already recommended (3182.). Nine moderately quick direct revolutions (*i. e.* as the hands of the clock) gave as the average of many experiments 23°·87, and nine reverse revolutions gave 23°·37; the mean of these is 23°·62 for the nine revolutions of the rectangle, and therefore 2°·624 per revolution. Now the six quick revolutions (3196.) gave 15°·66, which is 2°·61 per revolution, and the twelve quick revolutions gave 31°·33, which is also 2°·61 per revolution; and these results of 2°·624, 2°·61, and 2°·61, are very much in accordance, and give great confidence in this method of investigating magnetic forces*.

3198. A rectangle was prepared of the same length (4 feet) of the same wire, but the sides were respectively 8 and 16 inches (fig. 6), so that when revolving the intersecting parts should be only 8 inches in length instead of 12. The area of the rectangle was necessarily 128 square inches instead of 144. This rectangle showed the same difference of quick and slow rotations as before (3196.). When nine direct revolutions were made, the result was 20°·87 swing. Nine reverse revolutions gave an average of 20°·25 swing; the mean is 20°·56, or 2°·284 per revolution. A third rectangle was prepared of the same length and kind of wire, the sides of which were respectively 8 and 16 inches long (fig. 7), but now so revolved that the intersecting parts were 16 inches, or twice as long as before; the area of the rectangle remained the same, *i. e.* 128 inches. The like effect of slow and quick revolutions appeared as in the former cases (3196. 3198.). Nine direct revolutions gave as the average effect 20°·75; and nine reverse revolutions produced 21°·375; the mean is 21°·06, or 2°·34 per revolution.

Fig. 6. Fig. 7.



3199. Now 2°·34 is so near to 2°·284, that they may in the present state of the investigation be considered the same. The little difference that is evident, was, I suspect, occasioned by centrifugal power throwing out the middle of the longer intersecting parts during the revolution. The coincidence of the numbers shows, that the variation in the arrangement of the rectangle and in the length of the parts of the wires intersecting the lines of magnetic force, have had no influence in altering the result, which, being dependent alone on the number of lines of force intersected, is the same for both; for the area of the rectangles is the same. This is still further shown by comparing the results with those obtained with the square. The area in

* $\sin\frac{15\cdot66}{2}=\sin 7\cdot83=\sin 7^\circ\,49'\cdot8=\cdot1362343$ $\qquad \frac{1362343}{6}=\cdot0227057$

$\sin\frac{23\cdot62}{2}=\sin 11\cdot81=\sin 11\,48\cdot6=\cdot2047069$ $\qquad \frac{2047069}{9}=\cdot0227474$

$\sin\frac{31\cdot33}{2}=\sin 15\cdot665=\sin 15\,40=\cdot2700403$ $\qquad \frac{2700403}{12}=\cdot0225034$

that case was 144 square inches, and the effect per revolution 2°·61. With the long rectangles the area is 128 square inches, and the mean of the two results is 2°·312 per revolution. Now 144 square inches is to 128 square inches as 2°·61 is to 2°·32; a result so near to 2°·312 that it may be here considered as the same; proving that the electric current induced is directly as the lines of magnetic force intersected by the moving wire*.

3200. It may also be perceived that no difference is produced when the lines of force are chiefly disposed in the direction of the motion of the wire, or else, chiefly in the direction of the length of the wire; *i. e.* no alterations are occasioned by variations in the *velocity* of the motion, or of the length of the wire, provided the amount of lines of magnetic force intersected remains the same.

3201. Having a square on the frame 12 inches in the side but consisting of copper wire 0·1 of an inch in thickness, I obtained the average result of many observations for one, two, three, four and five revolutions of the wire.

One revolution gave	7°	equal to 7° per revolution.
Two revolutions gave	13·875	equal to 6·937 per revolution.
Three revolutions gave	21·075	equal to 7·025 per revolution.
Four revolutions gave	28·637	equal to 7·159 per revolution.
Five revolutions gave	37·637	equal to 7·527 per revolution.

These results are exceedingly close upon each other, especially for the first 30°, and confirm several of the conclusions before drawn (3189. 3199.) as to the indications of the instrument, the amount of the curves, &c.†

* Oblong rectangles of 128 square inches area give a mean of 20°·81 (3198.). The rectangle of 144 square inches gave a mean of 23°·62 (3197.).

$$\sin\frac{20{\cdot}81}{2} = \sin 10{\cdot}405 = \sin 10^\circ\,24'\,3'' = {\cdot}1806049$$

$$\sin\frac{23{\cdot}62}{2} = \sin 11{\cdot}81 = \sin 11\;48{\cdot}6 = {\cdot}2047069$$

$$\frac{128}{144} = \frac{8}{9} \qquad {\cdot}1806049 \times 9 = 1{\cdot}6254441 \qquad {\cdot}2047069 \times 8 = 1{\cdot}6376552$$

Or thus: $\frac{{\cdot}1806049}{8} = {\cdot}0225756$ $\quad \frac{{\cdot}2047069}{9} = {\cdot}0227452$

† Differences.

$$\sin\frac{7}{2} = \sin 3^\circ{\cdot}30 = {\cdot}0610485 \qquad {\cdot}0610485$$

$$\sin\frac{13{\cdot}875}{2} = \sin 6{\cdot}9375 = \sin 6^\circ\,56'{\cdot}25 = {\cdot}1207866 \qquad {\cdot}0597381 \qquad \frac{{\cdot}1207866}{2} = {\cdot}0603933$$

$$\sin\frac{21{\cdot}075}{2} = \sin 10{\cdot}5375 = \sin 10\;32{\cdot}25 = {\cdot}1828790 \qquad {\cdot}0620924 \qquad \frac{{\cdot}1828790}{3} = {\cdot}0609596$$

$$\sin\frac{28{\cdot}637}{2} = \sin 14{\cdot}3185 = \sin 14\;19{\cdot}11 = {\cdot}2473119 \qquad {\cdot}0644329 \qquad \frac{{\cdot}2473119}{4} = {\cdot}0618279$$

$$\sin\frac{37{\cdot}637}{2} = \sin 18{\cdot}8185 = \sin 18\;49{\cdot}11 = {\cdot}3225714 \qquad {\cdot}0752595 \qquad \frac{{\cdot}3225714}{5} = {\cdot}0645142$$

3202. At another time I compared the effect of equable revolutions with other revolutions very irregular in their rates, the motion being sometimes even backwards and continually differing in degree by fits and starts, yet always so that within the proper time a certain number of revolutions should have been completed. The rectangle was of wire 0·2 of an inch thick; the mean of many experiments, which were closely alike in their results, gave for two smooth, equable revolutions 17°·5, and also for two irregular uncertain revolutions the same amount of 17°·5.

3203. The relation of the current produced to the mass of the wire was then examined; a relation, which has been investigated on a former occasion by loops and small magnets (3133.)*. For the present purpose two other equal squares were prepared, each a foot in the side, but the copper wire of which they consisted was respectively 0·1 and 0·2 of an inch in diameter; so that with the former rectangle they formed a series of three, having the same size, shape and area, but the masses of the moving wire increasing in the proportion of one, four and sixteen. When the rectangle of 0·1 wire was employed, six direct revolutions gave an average result of 41°·75, and six to the left gave 46°·25; the mean of the two is 44°, and this divided by 6 gives 7°·33 as the deflection per revolution. Again, three direct revolutions gave 20°·12, and three reverse revolutions 23°·1; the mean being 21°·61, and the deflection per revolution 7°·20. This is very close to the former result with six revolutions, namely 7°·33, and is a large increase upon the effect of the rectangle of wire 0·05 in diameter, namely 2°·61; nevertheless, it is not as 4 : 1; nor could such a result be expected, inasmuch as the mass of the chief conductor remained the same (3137.). When the results are compared with those made with like wires in the form of loops, they are found to be exceedingly close; in that case the results were as 16° to 44°·4 (3136.), which would accord with a ratio in the present case of 2°·61 to 7°·26; and it is as 2°·61 to 7°·242, almost identical.

3204. The average of the direct and reverse revolutions is seen above to differ considerably, *i. e.* up to 4° and 5° in the higher case. This does not indicate any error in principle, but results simply from the circumstance, that when the needles were quiescent in the galvanometer, they stood a little on one side of zero (3182.). I did not wish to adjust the instrument at the time, as I was watching for spontaneous alterations of the zero place, and prefer giving the numbers as they came out in the graduation, to any pen-and-ink correction of the notes.

3205. The third square of 0·2 wire gave such large swings, that I employed only a small number of revolutions. Three direct revolutions gave an average of 25°·58; three reverse revolutions gave 28°·5; the mean is 27°·04, and the amount per revolution 9°·01. Again, two direct revolutions gave 17°·5; two reverse revolutions gave 18°; the mean is 17°·75, and the amount per revolution 8°·87; the mean of the two final

* See a corresponding investigation by Christie. Philosophical Transactions, 1833, p. 120.

results is 8°·94, and is again an increase on the effect produced by the preceding rectangle of wire, only half the diameter of the present. This thickness of wire was also employed formerly as a loop (3136.); and if we compare the results then obtained with the present results, it is remarkable how near they approach to each other; a circumstance which leads to great confidence in the principles and practice of both forms of examination. When wires having masses in the proportion of 1 : 4 and 16 were employed as loops, the currents indicated by the galvanometer were as 1·00, 2·77, and 3·58; now that they are employed as rectangles subject to the earth's magnetic power, they are as 1·00, 2·78, and 3·45*.

3206. I formed a square, 12 inches in the side, of four convolutions of copper wire 0·05 of an inch in diameter; the single wire which formed it was consequently 16 feet long. Such a rectangle will, in revolving, intersect the same number of lines of magnetic force as the former rectangle made with wire 0·1 in diameter (3203.); there will also be the same mass of wire intersecting the lines, but, as a conductor, the first wire has in respect of diameter, only one-fourth the conducting power of the second; and then, to increase the obstruction, it is four times as long. Six direct revolutions gave an average result of 20°·6, and six reverse revolutions 19°·7; the mean is 20°·15, and the proportion per revolution 3°·36. With the other rectangle having equal area and mass, but a single wire (3203.), the result per revolution was 7°·26; being above, though near upon twice as much as in the present case. Hence for such an excellent conducting galvanometer as that described (3123. 3178.), the moving wire had better be as one single thick wire rather than as many convolutions of a thin one. If it be, under all variations of circumstances, the same wire for the same area, then, of course, two or more convolutions are better than one.

3207. It was to be expected, however, that the thin wire rectangle would produce a current of more *intensity* than that in the thick wire, though less in quantity; and to prove this point experimentally, I connected the two rectangles in succession with RUHMKORFF's galvanometer (3086.), having wire only $\frac{1}{13}$th of an inch in diameter. That of the single thick wire now gave only 1°·66 of swing for twelve revolutions of the rectangle, or 0°·138 per revolution; whilst the other of four convolutions of thin

* $\sin\frac{27\cdot04}{2}=\sin 13°\cdot52=\sin 13°\ 31'\cdot2=\cdot2337848$ $\quad\frac{\cdot2337848}{2}=\cdot0779283$. The square 12 inches side, of wire 0·05 in diameter, gave for six revolutions (3196. 3197.) ·0227057 as $\sin\frac{1}{2}$ A for one revolution. A like square of wire 0·10 in diameter gave for five revolutions (3021.) $\frac{\cdot3225714}{2}=\cdot06451428$ as $\sin\frac{1}{2}$ A for one revolution. A like square of wire 0·20 in diameter gave ·0779283 as $\sin\frac{1}{2}$ A for one revolution

$$\frac{\cdot06451428}{\cdot0227057}=2\cdot841. \qquad \frac{\cdot0779283}{\cdot0227057}=3\cdot432.$$

wire gave for twelve revolutions 7°·33, or 0°·61 per revolution. Now the needles of the two instruments were not very different in weight and other circumstances, so that without pretending to an accurate comparison, we may still perceive an immense falling-off in both cases, due to the obstruction of the fine wire in the RUHMKORFF's galvanometer; for the thick wire it is from 7°·26 to 0°·138, and for the thin wire from 3°·36 to 0°·610. Still the thin wire rectangle has lost far less proportionately in power than the other; and by this galvanometer is above four times greater in effect than the rectangle of thicker wire. Of the thick wire effect less than a *fiftieth* passes the fine wire galvanometer, all the rest is stopped; of the fine wire effect more than ten times this proportion, or between a fourth and a fifth (because of the higher intensity of the current), surmounts the obstruction presented by the instrument. The quantity of electricity which really passes through the fine wire galvanometer is of course far less than in the proportion indicated above. The thick wire coil makes at the utmost four convolutions about the needles, whereas in the fine wire coil there are probably four hundred or more; so that the electricity which really travels forward as a current, is probably not a hundredth part of that which would be required to give an equal deflection in the thick wire galvanometer. Such a circumstance does not disturb the considerations with respect to the relative intensity of the magneto-electric currents from the two rectangles, which have been stated above.

3208. A large square was now constructed of copper wire 0°·2 of an inch in diameter. The square was 36 inches in the side, and therefore consisted of 12 feet of wire, and inclosed an area of 9 square feet; it was attached to the commutator by expedients, which, though sufficient for the present, were not accurate in the adjustments. It produced a fine effect upon the thick wire galvanometer (3178.); for one revolution caused a swing deflection of 80° or more; and when its rotation was continuous the needles were permanently deflected 40° or 50°. It was very interesting to see how, when this rectangle commenced its motion from the horizontal plane, the current increased in its intensity and then diminished again, the needles showing, that whilst the first 10° or 20° of revolution were being passed, there was very little power exerted in them; but that when it was towards, or near the 90°, the power was great; the wires then intersecting the lines of force nearly at right angles, and therefore, with an equal velocity, crossing the greatest number in a given time. It was also very interesting, by the same indications, to see the two chief impulses (3192.) given in one revolution of the rectangle. Being large and massive in proportion to the former wires, more time was required for a rotation than before, and the point of *time* or *velocity* of rotation became more essential. One rotation in a second was as much as I could well produce. A speed somewhat less than this was easy, convenient and quick enough; it gave for a single revolution near 80°, whilst a revolution with one-half or one-third the velocity, or less, gave only 60°, 50°, or even smaller amounts of deflection.

3209. Observations were now made on the measurement of one rotation having an easy quick velocity. The average of fifteen observations to the right, which came very near to each other, was 78°·846; the average of seventeen similar observations to the left was 78°·382; and the mean of these results, or 78°·614, I believe to be a good first expression for this rectangle. On measuring the distances across after this result, I found that in one direction, *i. e.* across between the intersecting portions of wire, it was rather less than 36 inches; having therefore corrected this error, I repeated the observations and obtained the result of 81°·44. The difference of 2°·83, I believe to be a true result of the alteration and increase of the area on making it more accurately 9 square feet; and it is to me an evidence of the sensibility and certainty of the instrument.

3210. As the two impulses upon the needles in one revolution (3208.) are here sensibly apart in time, and as the needle has as evidently and necessarily left its first place before the second impulse is impressed upon it, so, that second impulse cannot be so effectual as the first. I therefore observed the results with half a revolution, and obtained a mean of 41°·37 for the effect. This number evidently belongs to the first of the two impulses of one revolution; and if we subtract it from 81°·44, it gives 40°·07 as the value of the second impulse under the changed place of the needle. This difference of the two impulses of one revolution, namely 41°·37 and 40°·07, is in perfect accordance with the results that were to be expected.

3211. The square of this same copper wire, 0·2 in thickness, employed on a former occasion (3205.), had an area of one square foot, so that then the lines of force affected or affecting the moving wire, were one-ninth part of what they are in the present case: the effect then was 8°·94 per revolution. If, in comparing these cases, we take the ninth part of 81°·44, it gives 9°·04; a number so near the former, that we may consider the two rectangles as proving the same result, and at the same time the truth of the statement, that the magneto-electric current evolved is as the amount of lines of force intersected. A ninth part of the result with the large rectangle (78°·614), before its area was corrected, is 8°·734; so that the one is above and the other below the amount of the 12-inch rectangle. As that was not very carefully adjusted, nor indeed any of the arrangements made as yet with extreme accuracy, I have little doubt that with accurately adjusted rectangles the results would be strictly proportional to the areas*.

* The 9 square feet rectangle gave 81°·44 $\sin \frac{81 \cdot 44}{2} = \sin 40 \cdot 72 = \sin 40^\circ\ 43' \cdot 2 = \cdot 6523630$; or taking 41°·37 for the half revolution for $\frac{1}{2}$ A (3210.) $\sin 41 \cdot 37 = \sin 41^\circ\ 22' \cdot 2 = \cdot 6609190$, which divided by nine give ·073435 as the force per square foot. The 1 square foot rectangle of like wire (3205.) gave ·07714, or ·07793 as the force of one revolution; the first of which is ·00370 more than $\frac{1}{9}$ of the measure of the effect of the large square; the difference being about $\frac{1}{29}$ of ·07714, or the whole force of one revolution.

3212. The moving wire, in place of being formed into a rectangle, may be adjusted as a ring; and then the advantage is obtained of the largest area which a given length of wire can inclose, and therefore for a uniform wire, the obstruction to the induced current, as respects its conduction, is the least. Small rings of one or several convolutions will probably be very valuable in the examination of small and local magnets under different circumstances. One consisting of ten spirals of copper wire 0°·032 of an inch in diameter, containing 49 inches, in a ring about 1·5 inch in diameter, gave but small results under the earth's influence; but when brought near a horseshoe magnet told, in its effects for every difference in distance or in position. A single ring 4 inches in diameter, being made of a convolution of copper wire 0·2 in thickness, was employed with the earth's magnetic force as before; it gave as the average of six revolutions many times repeated 5°·995, or 0°·999 per revolution. For twelve revolutions it gave a mean of 12°·375, or 1°·031 per revolution*; the mean of the two results with such different numbers of revolutions being 1°. Another ring, consisting of twenty-six convolutions of copper wire 0·04 of an inch in diameter, was constructed and had a mean diameter of 3·6 or 3·7 inches; it contained 300 inches in length of wire. So the masses of the metal in the two rings are nearly the same, but the latter wire is singly only $\frac{1}{25}$th of the mass of the former. It gave for twelve revolutions a mean of 6°·25, or 0°·52 per revolution. With the earth's power and the thick wire galvanometer, it gave therefore little more than half the result of the single thick wire ring. We know from former considerations (3206.), that if the 300 inches had been made into one single ring, it would have given a very high effect compared to the present.

3213. The application of the principle of the moving wire in the form of a revolving rectangle, makes the investigation of *conducting* power, and the results produced by difference in the nature of the *substance*, or in diameter, *i. e. mass*, or in *length*, very easy; and the obstruction offered by those parts, which moving not across but parallel to the lines of force (3071.), have no exciting action but perform the part of conductors merely, might be greatly removed by making them massive. They might be made to shift upon the axle so as to bear adjustment for different lengths of wires, and the commutator might in fact be made to a large extent a general instrument.

3214. In looking forward to further applications of the principle of the moving wire, it does not seem at all unlikely that by increased delicacy and perfection of the instrument, by increased velocity, by continued motion for a time in one direction and then reversal of the revolution with the reversal of the direction of the swing, &c., it may be applied with advantage hereafter to the investigation of the earth's magnetic force in different latitudes and places. To obtain the maximum

* $\sin \frac{5°·995}{2} = \sin 2°·9975 = \sin 2° 59'·85 =$ ·0522925

$\sin \frac{12°·375}{2} = \sin 6°·1875 = \sin 6\ 11'·25 =$ ·1077825 $\qquad \frac{·1077825}{2} =$ ·0538912.

effect, the axis of rotation must be perpendicular to the lines of force, *i. e.* the dip. It would even be possible to search for the *direction* of the lines of force, or the dip, by making the axis of rotation variable about the line of dip, adjusting it in two directions until there was no action at the galvanometer, and then observing the position of the axis; a double commutator would be required corresponding to the lines of adjustment, but that is of very simple construction.

§ 36. *On the amount and general disposition of the Forces of a Magnet when associated with other magnets.*

3215. Prior to further progress in the experimental development by a moving wire of the disposition of the lines of magnetic force pertaining to a magnet, or of the physical nature of this power and its possible mode of action at a distance, it became quite essential to know what change, if any, took place in the amount of force possessed by a perfect magnet, when subjected to other magnets in favourable or adverse positions; and how the forces combined together, or were disposed of, *i. e.* generally, and in relation to the principle already asserted and I think proved, that the power is in every case definite under those different conditions. The representation of the magnetic power by *lines of force* (3074.), and the employment of the moving wire as a test of the force (3076.), will I think assist much in this investigation.

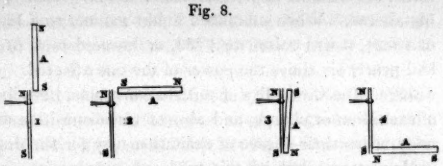
3216. For such a purpose an ordinary magnet is a very irregular and imperfect source of power. It not only, when magnetized to a given degree, is apt by slight circumstances to have its magnetic power diminished or exalted, in a manner which may be considered for the time, permanent; but if placed in adverse or favourable relations to other magnets, frequently admits of a considerable temporary diminution or increase of its power externally, which change disappears as soon as it is removed from the neighbourhood of the dominant magnet. These changes produce corresponding effects upon the moving wire, and they render any magnet subject to them unfit for investigation in relation to definite power. Unchangeable magnets are, therefore, required, and these are best obtained, as is well known, by selecting good steel for the bars, and then making them exceedingly hard; I therefore procured some plates of thin steel 12 inches long and 1 inch broad, and making them as hard as I could, afterwards magnetized them very carefully and regularly, by two powerful steel bar-magnets, shook them together in different and adverse positions for a little while, and then examined the direction of the forces by iron filings. Small cracks and irregularities were in this way detected in several of them; but two which were very regular in the disposition of their forces were selected for further experiment, and may be distinguished as the subjected magnets D and E.

3217. These two magnets were examined by the moving loop precisely in the manner before described (3133.), *i. e.* by passing the loop over one of the poles, observing the swing, removing it, and again observing the swing and taking an average of many results; the process was performed over both poles at different times. The loop

contained 7·25 inches in length of copper wire 0·1 of an inch in diameter, and was of course employed in all the following comparative experiments; the distance of the loop and magnets from the galvanometer was 9 feet. For one passage over the pole either on or off, *i. e.* for one intersection of the lines of force of the magnet D, the galvanometer deflection was 8°·36. For one intersection of the lines of force of the other bar E, the deflection was 8°·78. The two bars were then placed side by side with like poles together, and afterwards used as one magnet; their conjoined power was 16°·3, being only 0°·84 less than the sum of the powers of the two when estimated separately. This indicates that the component magnets do affect, and in this position reduce, each other somewhat; but it also shows how small the effect is as compared with ordinary magnets (3222.).

3218. The compound magnet D E (3217.) was now subjected to the close action of another magnet, sometimes under adverse, and at other times under favourable conditions; and was examined by the loop as to the sum of its power (not the direction) under these circumstances. For this purpose it was fixed, and another magnet A brought near, and at times in contact with it, in the positions indicated by the figure 8; the loop in each case being applied many times to D E, that a correct average of its power might be procured. The dominant magnet A was much the stronger of the two, having the power indicated by a swing deflection of 25°·74.

Fig. 8.



3219. When the relative position of the magnets was as at 1, then the power of D E was 16°·37; when as at 2, the power was 16°·4; when as at 3, it was 18°·75; and when as at 4, it was 17°·18. All these positions are such as would tend to raise, by induction, the power of the magnet D E, and they do raise it above its first value, which was 16°·3; but it is seen at once how little the first and second positions elevate it; and even the third, which presents the most favourable conditions, only increases the power 2°·45, which falls again in the fourth position.

3220. Then the dominant magnet A was placed in the same positions, but with the ends reversed, so as to exert an adverse or depressing influence; and now the results with D E were as follows:—

Position 1	15°·37
Position 2	15·68
Position 3	15·37
Position 4	16·06

All these are a little below the original force of D E, or 16°·3, as they ought to be, and show how slightly this hard bar-magnet is affected.

3221. A soft iron bar, now applied in the first, second and third positions instead of the magnet A, raised D E to the following values respectively, 16°·24, 16°·43, and 18°.

3222. When an ordinary bar-magnet was employed instead of the hard magnet D E, great changes took place. Thus a bar B, corresponding to bar A in size and general character, was employed in place of the hard magnet. Alone, B had a power of 14°·83, but when associated adversely with A, as in position 3 (3218.), its power fell to 7°·87, being reduced nearly one-half. This loss was chiefly due to a coercion internally, and not to a permanent destruction of the state of magnet B; for when A was removed, B rose again to 13°·06. When B was laid for a few moments favourably on A and then removed, it was found that the latter had been raised to a permanent external action of 15°·25.

3223. A very hard steel bar 6 inches long, 0·5 broad and 0·1 in thickness, given to me by Dr. Scoresby, was magnetized and then found, by the use of the loop, to have a value at my galvanometer of 6°·88 (3189.). It was submitted in position 2 to a compound bar-magnet like D E, having a power of 11°·73, or almost twice its own force, but whether in the adverse or the favourable position, its power was not sensibly altered. When submitted in like manner to a 12-inch bar-magnet having a force of 40°·21, it was raised to 7°·53, or lowered to 5°·87, but here the dominant magnet had nearly six times the power of the one affected.

3224. The variability of soft steel magnets, both in respect of their *absolute* degree of excitation or charge, and also of the disposition of the force externally and internally, when their degree of excitation may for the time be considered as the same, is made very manifest by this mode of examination; and the results agree well with our former knowledge in this respect. It is equally manifest, that hard and invariable magnets are requisite for a correct and close investigation of the disposition and characters of the magnetic force. A common soft bar-magnet may be considered as an assemblage of hard and soft parts, disposed in a manner utterly uncertain; of which some parts take a much higher charge than others, and change less under the influence of external magnets; whilst, because of the presence of other parts within, acting as the keeper or submagnet, they may seem to undergo far greater changes than they really do. Hence the value of these hard and comparatively unchangeable magnets which Scoresby describes.

3225. From these and such results, it appears to me, that with perfect, unchangeable magnets, and using the term *line of force* as a mere representant of the force as before defined (3071. 3072.), the following useful conclusions may be drawn.

3226. Lines of force of different magnets in favourable positions to each other coalesce.

3227. There is no increase of the total force of the lines by this coalescence; the section between the two associated poles gives the same sum of power as that of the section of the lines of the invariable magnet when it is alone (3217.). Under these

circumstances there is, I think, no doubt that the external and internal forces of the same magnet have the same relation and are equivalent to each other, as was determined in a former part of these Researches (3117.); and that therefore the equatorial section, which represents the sum of forces or lines of forces passing through the magnet, remains also unchanged (3232.).

3228. In this case the analogy with two or more voltaic batteries associated end to end in one circuit is perfect. Probably some effect, correspondent to *intensity* in the case of the batteries, will be found to exist amongst the magnets.

3229. The increase of power upon a magnetic needle, or piece of soft iron placed between two opposite, favourable poles, is caused by concentration upon it of the lines which before were diffused, and not by the addition of the power represented by the lines of force of one pole to that of the lines of force of the other. There is no more power represented by all the lines of force than before; and a line of force is not more powerful because it coalesces with a line of force of another magnet. In this respect the analogy with the voltaic pile is also perfect.

3230. A line of magnetic force being considered as a closed circuit (3117.), passes in its course through *both* the magnets, which are for the time placed so as to act on each other favourably, *i. e.* whose lines coincide and coalesce. Coalescence is not the addition of one line of force to another *in power*, but their union in one common circuit.

3231. A line of force may pass through many magnets before its circuit is complete; and these many magnets coincide as a case with that of a single magnet. If a thin bar-magnet 12 inches long be examined by filings (3235.), it will be found to present the well-known beautiful system of forces, perfectly simple in its arrangement. If it be broken in half, without being separated, and again examined, the manner in which, from the destruction of the continuity, the transmission of the force at the equator is interfered with, and many of the lines, which before were within are made to appear externally there, is at once evident, Plate IX. fig. 6. Of those lines, which thus become external, some return back to the pole which is nearest to the new place, at which the lines issue into the air, making their circuit through only one of the halves of the magnet; whilst others proceed onward by paths more or less curved into the second half of the magnet, keeping generally the direction or polarity which they had whilst within the magnet, and complete their circuit through the two. Gradually separating the two halves, and continuing to examine the course of the lines of force, it is beautiful to observe how more and more of the lines which issue from the two new terminations, turn back to the original extremities of the bar, fig. 7, and how the portion which makes a common circuit through the two halves diminishes, until the halves are entirely removed from each other's influence, and then become two separate and independent magnets. The same process may be repeated until there are many magnets in place of one.

3232. All this time the amount of lines of force is the same if the fragments of the

bar preserve their full state of magnetism; *i. e.* the sum of lines of force in the equator of *either* of the new magnets is equal to the sum of lines of force in the equator of the original unbroken bar. I took a steel bar 12 inches long, 1 inch broad and 0·05 of an inch thick, made it very hard, and magnetized it to saturation by the use of soft iron cores and a helix; its power was 6°·9. I broke it into two pieces nearly in the middle, and found the power of these respectively 5°·94 and 5°·89; indicating a fall not more than was to be expected considering the saturated state of the original magnet. When these halves were placed side by side, with like poles together as a compound magnet, they had a joint power of 11°·06, which, though it shows a mutual quelling influence, is not much below the sum of their powers ascertained separately. All this is in perfect harmony with the voltaic battery, where lines of dynamic electric force are concerned. If, as is well known, we separate a battery of 20 pair of plates into two batteries of 10 pair, or 4 batteries of 5 pair, each of the smaller batteries can supply as much dynamic electricity as the original battery, provided no sensible obstruction be thrown into the course of the lines, *i. e.* the path of the current.

3233. When magnets are placed in an adverse position, as neither could add power to the other in the former case, so now each retains its own power; and the lines of magnetic force represent this condition accurately. Two magnets placed end to end with like poles together are in this relation; so also are they if placed with like poles together side by side. In the latter case the two acting as one compound magnet, give a system of lines of force equal to the sum of the two separately (3232.), minus the portion which, as in imperfect magnets, is either directed inwards by the softer parts or ceases to be excited altogether.

§ 37. *Delineation of Lines of Magnetic Force by iron filings.*

3234. It would be a voluntary and unnecessary abandonment of most valuable aid, if an experimentalist, who chooses to consider magnetic power as represented by lines of magnetic force, were to deny himself the use of iron filings. By their employment he may make many conditions of the power, even in complicated cases, visible to the eye at once; may trace the varying direction of the lines of force and determine the relative polarity; may observe in which direction the power is increasing or diminishing; and in complex systems may determine the neutral points or places where there is neither polarity nor power, even when they occur in the midst of powerful magnets. By their use probable results may be seen at once, and many a valuable suggestion gained for future leading experiments.

3235. Nothing is simpler than to lay a magnet upon a table, place a flat piece of paper over it, and then, sprinkling iron filings on the paper, to observe the forms they assume. Nevertheless, to obtain the best and most generally useful results, a few particular instructions may be desirable. The table on which the magnet is laid should be quite horizontal and steady. Means should be taken, by the use of thin boards or laths, or otherwise, to block up round the magnet, so that the paper which

is laid over it should be level. The paper should be without any cockle or bend, and perfectly flat, that the filings may be free to assume the position which the magnet tends to give them. I have found well-made cartridge or thin drawing-paper good for the purpose. It should not be too smooth in ordinary cases, or the filings, when slightly agitated, move too freely towards the magnet. With very weak or distant magnets I have found silvered paper sometimes useful. The filings should be clean, *i. e.* free from much dirt or oxide; the latter forms the lines but does not give good delineations. The filings should be distributed over the paper by means of a sieve more or less fine, their quantity being partly a matter of taste. It is to be remembered, however, that the filings disturb in some degree the conditions of the magnetic power where they are present, and that in the case of small magnets, as needles, a large proportion of them should be avoided. Large and also fine filings are equally useful in turn, when the object is to preserve the forms obtained. For the distribution of the latter it is better to use a fine sieve with the ordinary filings than to separate the filings first: a better distribution on the paper is obtained. The filings being sifted evenly on the paper, the latter should be tapped very lightly by a small piece of wood, as a pen-holder; the taps being applied wherever the particles are not sufficiently arranged. The taps must be perpendicularly downwards, not obliquely, so that the particles, whilst they have the liberty of motion, for an instant are not driven out of their places, and the paper should be held down firmly at one corner, so as not to shift right or left; the lines are instantly formed, especially with fine filings.

3236. The designs thus obtained may be fixed in the following manner, and then form very valuable records of the disposition of the forces in any given case. By turning up two corners of the paper on which the filings rest, they may be used as handles to raise the paper upwards from the magnet, to be deposited on a flat board or other plane surface. A solution of one part of gum in three or four of water having been prepared, a coat of this is to be applied equably by a broad camel-hair pencil, to a piece of cartridge paper, so as to make it fairly wet, but not to float it, and after wafting it through the air once or twice to break the bubbles, it is to be laid carefully over the filings, then covered with ten or twelve folds of equable soft paper, a board placed over the paper, and a half-hundred weight on the board for thirty or forty seconds. Or else, and for large designs it is a better process, whilst the papers are held so that they shall not shift on each other, the hand should be applied so as to rub with moderate pressure over all the surface equably and in one direction. If, after that, the paper be taken up, all the filings will be found to adhere to it with very little injury to the forms of the lines delineated; and when dry they are firmly fixed. If a little solution of the red ferroprussiate of potassa and a small proportion of tartaric acid be added to the gum-water, a yellow tint is given to the paper, which is not unpleasant; but besides that, prussian blue is formed under every particle of iron; and then when the filings are purposely or otherwise dis-

placed, the design still remains recorded. When the designs are to be preserved in blue only, the gum may be dispensed with and the red ferroprussiate solution only be used.

3237. It must be well understood that these forms give no indication by their appearance of the relative strength of the magnetic force at different places, inasmuch as the appearance of the lines depends greatly upon the quantity of filings and the amount of tapping; but the direction and forms of the lines are well given, and these indicate, in a considerable degree, the direction in which the forces increase and diminish.

3238. Plate IX. fig. 1, shows the forms assumed about a bar-magnet. On using a little electro-magnet and varying the strength of the current passed through it, I could not find that a variation in the strength of the magnet produced any alteration in the forms of the lines of force external to it. Fig. 2 shows the lines over a pole, and fig. 3 those between contrary poles. The latter accord with the magnetic curves, as determined and described by Dr. ROGET and others, with the assumption of the poles as centres of force. The difference between them and those belonging to a continuous magnet, shown in fig. 1, is evident. Figs. 4, 5 show the lines produced by short magnets. In the latter case the magnet was a steel disc about one inch in diameter and 0·05 in thickness. Fig. 6 shows the result when a bar-magnet is broken in half, but not separated. Fig. 7 shows the development of the lines externally at the two new ends as the halves are more and more separated (3231.). Figs. 8, 9 and 10 present the results, with the two halves or new magnets in different positions. Figs. 11, 12, 13 and 14 show the results with disc magnets. Fig. 15 shows the condition of a system of magnetic forces when it is inclosed by a larger one, and is contrary to it. Fig. 16 shows the coalescence of the lines of force (3226.) when the magnets are so placed that the polarities are in accordance.

3239. Fig. 17 exhibits the lines of force round a vertical wire carrying a current of electricity. Whether the wire was thick or thin appeared to make no difference as to the intensity of the forces, the current remaining the same. Fig. 18 represents the lines round two like currents when within mutual influence. Fig. 19 shows the result when a third current is introduced in the contrary direction. Fig. 20 presents the transition to a helix of three convolutions. Fig. 21 indicates the direction of the lines within and outside the end of a cylindrical helix, on a plane through its axis. Fig. 22 presents the effect when a very small soft iron core is within the helix.

3240. Figs. 23 and 24 give an experimental illustration of the principles which I have adopted in relation to atmospheric magnetism and the general cause of the daily variations, &c. (2864. 2917.). A hemisphere of pure nickel presented to me by Dr. PERCY, was supported with its flat face uppermost, and a large ring arranged round it to carry paper, which, resting both on the ring and the nickel, could then have iron filings sprinkled and arranged in form on it. The end of a bar-magnet in the same horizontal plane was adjusted about 2 inches from the nickel, and thus the

forms of the lines of force associated with this pole could be determined over the place of the nickel hemisphere, under different circumstances, or even when it was removed. When the nickel was away, the forms of the lines of force were as in fig. 23; when the nickel was there, they were as in fig. 24. The application of a spirit-lamp to the nickel when in its place, raised its temperature to such a degree (above 600° FAHR.) that it lost its ordinary magnetic condition; and then the forms of the lines of force, as shown by filings, were the same as if the nickel was away. Removing the lamp, I was able to obtain the disposition of filings on successive pieces of paper, and as many as four results, like fig. 23, could be procured before the temperature had sunk so much as to cause the production of lines of force corresponding to fig. 24.

3241. These are exactly the same results with nickel as those I have assumed for the oxygen of the atmosphere. The change in the forms of the lines about the cooling nickel in this experiment are the same changes as those I have figured in the type globe of cooling air (2865. 2874.). Both nickel and oxygen are paramagnetic bodies, and change in the *same direction* by heating and cooling; and as the period of change with oxygen extends through degrees above and below common temperature (2861.), so inflections of the lines of force passing through the atmosphere, correspondent to those of the heating and cooling nickel, *must* take place to some extent. It is seen in the nickel results, that lines of force entirely outside of it do not for that reason continue an undeviating course, but are curved to and fro in consequence of the disposition of other lines within the nickel; a result, which, without reference to either one view or another of the physical action of the magnetic force, must be as true in the oxygen case as in the nickel case, because of the definite character of the magnetic force, whether represented by centres of action or by lines of power.

3242. Whether the amount of the deflection in the case of the atmosphere corresponds with the facts registered by observers, is a question which cannot be answered, I suppose, until we know the effect of very low temperatures upon the magnetic force of the atmosphere. In the nickel experiment the deflection is in places 30° or 40°; in nature the effect to be accounted for is not more than 13 or 14 minutes.

Royal Institution,
December 20, 1851.

XII. *On Symbolic Forms derived from the Conception of the Translation of a Directed Magnitude.* *By the Rev.* M. O'Brien, *M.A., Late Fellow of Caius College, Cambridge, and Professor of Natural Philosophy and Astronomy in King's College, London.*

Received April 21,—Read June 19, 1851.

PART I.

GENERAL INVESTIGATION OF THE SYMBOLIC FORMS.

(1.) THERE can be no doubt, that time and ingenuity have been often wasted in devising systems of notation, and new methods of algebraical representation, which have never proved of any service in advancing the cause of science. It is not surprising, therefore, that symbolical innovations, if they have not the strongest and most obvious reasons to recommend them, are generally received with little favour by mathematicians. At the same time, it must not be forgotten, that the mind has wonderfully enlarged its powers of research by the symbolization of its abstract conceptions, and that the various additions which have been made, from time to time, to mathematical notation, have contributed largely to the progress of physical investigation; witness, for instance, the applications of the negative sign, indices, logarithms, coordinate equations, the differential algorithm, &c.

A new notation, or a new application of an old notation, ought, in all cases, to be called for by some want in science, that is, by the existence of some important and often occurring conception for which there is no adequate, or at least no sufficiently general mode of representation. It should be neither artificial nor complicated, but natural and simple: it should also be based on principles of established authority, and framed according to allowed precedents. And, lastly, it should be capable of something more than mere elementary applications, and be recommended by its utility in the higher and more abstruse branches of science.

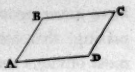
With these cautions before me, and on these grounds, I venture, in the present paper, to propose a new use of an old notation, which appears to me to supply a want of considerable importance, as I hope to show by the remarkable simplifications which it introduces into many difficult investigations. There is an operation, if I may so call it, of constant occurrence in Geometry and Physics, which consists in the *translation of a directed magnitude*, that is, the parallel motion of a magnitude possessing the property of direction, such, for example, as a force, a velocity, traced line, or the like. This translation, as it may be easily shown, is always an operation

of the kind called "*distributive*," and from its distributiveness all its properties in a great measure follow as necessary consequences. Now this is a fact of great importance to be borne in mind, a thing to which the truly mathematical adage "*when found make a note of*" fully applies. There is no notation actually in use for this purpose; but there are two nearly obsolete signs, which by an easy and natural generalization, and according to the most approved rules of mathematical interpretation, might be made to serve effectually as representatives of the effects produced generally by the translation of a directed magnitude. My design, in what follows, is to show this, and establish the laws according to which these signs are to be used in their enlarged signification. Afterwards I shall endeavour to justify the proposed innovation, if such it is to be considered, by showing its utility in a variety of cases.

(2.) *Instances which suggest the proposed symbolization.*—There are three elementary conceptions which have suggested to my mind the principles which it is the object of this paper to develope, and they will serve here as means of introducing the subject, and furnish the best foundation to build upon. They are the following:—

1st. *The generation of surface by the parallel motion of a right line*, of which the simplest instance is a parallelogram ABCD supposed to be generated by the motion of AB, parallel to itself, along AD.—

Fig. 1.



2ndly. *The effect produced on a rigid body by the translation of a force acting upon it* from one point A to another B, the direction of the force remaining unaltered; which effect, as is well known, consists in that peculiar tendency to motion that results from the action of the couple composed of the force at B, and a force equal and opposite to the original force at A.—3rdly. *The effect produced by the translation of a force resulting from the actual motion of its point of application*; which effect is now usually designated by the term *work*. If the point of application be supposed to describe the path ADB, the force all the time acting parallel to its original direction, a certain amount of work is accumulated in consequence of the translation of the force, and this is the effect I allude to.

Fig. 2.

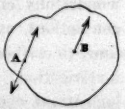
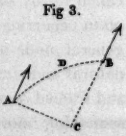


Fig 3.



Now in each of these cases the conception in the mind is that of *the effect produced by the translation of a directed magnitude*; and what is worthy of special remark is this, that in each of these cases the effect alluded to is represented by the *product of two factors, one* being *the translated magnitude*, and *the other the amount of translation* it undergoes. Thus, in the case shown by figure 1, the surface generated is denoted by the product of AB into the perpendicular or *lateral* distance between AB and CD; of which, AB is the translated magnitude, and the perpendicular the amount of translation that takes place *laterally*. In the case shown by figure 2, the effect is also represented by the product of the translated magnitude, that is, the force, into the amount of *lateral* translation. In the case shown by figure 3, if we suppose BC to be the direction of the force produced back-

ward to meet a perpendicular AC let fall from A upon it, the effect produced, that is, the work accumulated, is represented by the product of the translated magnitude (the force, namely,) into the amount of translation which takes place, not laterally, as before, but *longitudinally*, that is, *along* the direction of the force, which longitudinal translation is manifestly CB.

(3.) From these three instances the idea naturally arises of *some necessary connection* between the translation of a directed magnitude and the product of the two factors, the magnitude translated and the amount of translation; or, to say the least, there appears to be some ground for conceiving that the product in question may be the *proper form of notation* for representing the translation. And, secondly, the necessity of *distinguishing* between *lateral* and *longitudinal* translation is clearly indicated, inasmuch as the longitudinal effect is zero in the first two cases, while the lateral effect is zero in the third case. Taking my clue from these suggestions, I shall now proceed to explain my proposed method of notation; observing, that my object is to make it as general as possible consistently with definiteness and utility, and that, for this reason, I shall employ all the generalizations of the elementary algebraical signs which are now admitted by mathematicians.

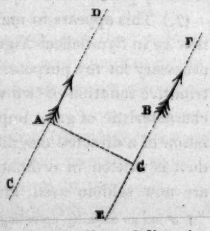
I shall also adopt, to a certain extent, the views of Symbolical Algebra taken by the late Mr. Gregory, and published by him in several papers, but especially in one read before the Royal Society of Edinburgh on the Foundations of Algebra. I may observe, however, that my proposed method of notation does not assume the correctness of these views, and might be enunciated independently of them; but they appear to my mind to form the most satisfactory theory of Symbolical Algebra.

I. Preliminary Definitions, Statements, etc.

(4.) *Directed Magnitude.*—I use this term to denote any of those magnitudes which we represent graphically by *arrows*; remarking that an arrow represents *three* things, viz. an *origin* or *point of application*, marked by its *feather-extremity*; a particular *magnitude* represented by its *length*; and a particular *direction* shown by the *barb*.

(5.) *Translation, Lateral and Longitudinal.*—"Translation" is the term employed to denote that peculiar and simplest change of position of a rigid body which consists in the parallel and equal motion of all its component particles. I shall use the same term to denote a change of position of a directed magnitude without change of direction, as is shown in fig. 4. The translation therefore of a directed magnitude consists in simple alteration of "*origin*," as from A to B in the figure.

Fig. 4.

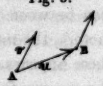


But this alteration of origin is of a *twofold* nature, being partly *lateral* and partly *longitudinal*. If CD be the indefinite line of direction

in which the arrow at A lies, and EF that in which the arrow at B lies, the translation from A to B consists of two distinct motions; namely, the shifting of the line of direction from CD to EF, and the shifting of the origin along the line of direction through a space amounting to GB. The former I shall call *lateral* and the latter *longitudinal* translation.

If v denote a directed magnitude which is supposed to be translated from A to B, and if u denote the line AB, I shall speak of the translation as *that of v along u*; a proper mode of expression, because every point of the representative arrow v undergoes a motion equivalent in magnitude and direction to u. The translation of v along u is lateral when the angle at A is 90°, and longitudinal when 0°.

Fig. 5.



(6.) *Distributiveness.*—If $f(x)$ be a function of x which possesses the property expressed by the equation

$$f(x)+f(x')=f(x+x'),$$

it is said to be a *distributive* function of x. If x be any *number* positive, negative, integral or fractional, it may be shown from this equation that

$$f(x)=Cx,$$

C denoting a quantity independent of x, namely the value of $f(x)$ when $x=1$. If x be not a number, but some symbol, whether of specific quantity or operation, the notation Cx has no meaning recognised in ordinary algebra. Hence, following the well-known precedent of indices*, we may generalize the meaning of Cx by assuming it to be the *symbolical form* for denoting *every function, $f(x)$, which is distributive.*

If we further suppose, that $f(x)$, and therefore C, is a distributive function of another independent variable y, we shall find that

$$C=C'y \quad \text{and} \quad \therefore Cx=C'xy.$$

Thus we may, by the same process of generalization, assume $C'xy$ to be the *symbolical form* for denoting *every function* of x and y *which is distributive* with regard to both x and y. C' here is manifestly the value of the function when $x=1$ and $y=1$. Now if we adopt C' to be the unit of the function, as, in fact, we do in many cases of ordinary products, the symbolic form for denoting the function becomes simply xy.

(7.) This appears to me to be the simplest and best method of defining the notation xy in Symbolical Algebra; though I need not avail myself of it here as it is not necessary for my purpose. All I require is some simple notation for denoting a distributive function of two variables; for, as I hope to show, this distributiveness is a characteristic of great importance to be distinctly "noted" in the case of the translation of a directed magnitude. Now there are three different forms in which a product is written in ordinary algebra, viz. xy $x.y$ and $x\times y$: of these, the latter two are now seldom used, and there is no necessity whatever for this redundancy of

* a^x has no meaning, according to its original definition, except x be a positive integer: but we give it a meaning by defining a to be the notation for every function $f(x)$ which possesses the property $f(x)f(y)=f(x+y)$.

forms for denoting the same thing. Instead, therefore, of inventing new symbols for representing distributive functions, I shall venture to appropriate the almost obsolete forms $x.y$ and $x \times y$ to the purpose. At the same time it must be borne in mind, that, according to the views of many eminent mathematicians, the product of x and y in Symbolical Algebra may be defined to be *any* distributive function of x and y, and thus the appropriation of $x.y$ and $x \times y$ here proposed is nothing more than a legitimate application of these forms.

(8.) I shall therefore assume $x.y$ and $x \times y$ to be symbolical forms for denoting distributive functions of x and y; in other words, I shall consider $x.y$ and $x \times y$ to be completely defined by the equations

$$x.y+x'.y=(x+x').y \qquad x \times y+x' \times y=(x+x') \times y$$
$$x.y+x.y'=x.(y+y') \qquad x \times y+x \times y'=x \times (y+y'),$$

just as the symbolic form a^m is completely defined by the equation $a^m a^n=a^{m+n}$.

(9.) Whether $x.y$ and $x \times y$ are "*Commutative*" functions of x and y, i. e. whether $x.y=y.x$, and $x \times y=y \times x$, does not appear from these defining equations, and therefore it must be decided by the particular nature of the quantity or operation which each of these forms is assumed to represent.

(10.) *Signification of the sign* $+$. In Symbolical Algebra the sign $+$ may be regarded as simply an abbreviation for the words "*together with*," and thus $u+v$ means simply u "together with" v, or u and v "*put together*." Now these words "*together with*" may be taken in a great variety of senses, as the following examples taken from ordinary algebra show, viz.

$3+5=8$, $\quad 3£+5s.=780d.$, $\quad 3+4\sqrt{-1}=(2+\sqrt{-1})^2$,
5 miles east $+$ 5 miles west $=0$, &c. &c.

In the first example $+$ means a "*putting together*" by simple numerical addition; in the second, a "*putting together*" of certain pieces of gold and silver, with reference to a certain conventional value set on them; in the third, a mere symbolical "putting together;" and so on. Hence it is clear that in using the sign $+$ as an abbreviation of the words "*together with*," the precise nature of the "*putting together*" is supposed to be understood in each case. I shall therefore define the notation, $u+v$, to mean, u *and* v *put together in a sense supposed to be understood.*

Now in some cases it is very important that the precise nature of the "*putting together*" denoted by the sign $+$ should be clearly understood, and therefore distinctly specified. This it will be necessary for me to do here with reference to two remarkable significations which have been given to the sign $+$.

(11.) The first is that signification given to $+$ in Symbolical Geometry. If u and v denote two lines of certain magnitudes and drawn in certain directions, then $u+v$ is assumed to denote u and v *put together* as in the figure 6; that is, the *beginning-point* or *origin* of v coinciding with the *end-point* (if I may so use the words) of u. The second signification

Fig. 6.

is that given to + in Symbolical Mechanics. If u and v denote two forces of certain magnitudes and directions, $u+v$ is assumed to denote u and v *put together* as in figure 7; that is, the *origin* of v coinciding, not with the *end-point*, as before, but with the *origin* of u. The distinction I allude to here is of considerable importance, and requires to be very closely attended to in applying lines to represent forces generally, as will appear. It might be well to distinguish these two significations of + by appropriate terms. I cannot think of any better words, for the purpose, than the two, "*successive*," and "*simultaneous*;" the *putting together* in fig. 6 is manifestly effected by tracing the two lines *in immediate succession*, while that in fig. 7 is a *simultaneous application at the same origin*. I shall therefore call the *putting together* in fig. 6 *successive addition*, and that in fig. 7 *simultaneous addition*.

Fig. 7.

(12.) *Signification of the sign* =. Like +, the sign = denotes *equivalence in a certain sense supposed to be understood*; thus in the example, $3£+5s.=780d.$, it denotes equivalence as regards the conventional value of certain coins. In Symbolical Geometry = has reference to the *change of position of the tracing-point* by which lines are supposed to be drawn. Thus if u, v, w denote three traced lines, the equation, $u+v=w$, means, that the tracing of $u+v$ is the same thing as the tracing of w, *so far as the change of position of the tracing-point is concerned*. In this sense it is clear, that w must be the third side of the triangle in fig. 8. In Symbolical Mechanics = has reference to *mechanical effect*. Thus if u, v, w be three forces, the equation, $u+v=w$, means, that *the mechanical effect of* u+v *is the same as that of* w; in other words, it means, that w is the *resultant* of u and v.

Fig. 8.

Fig. 9.

(13.) *Representation of Forces by Lines.* The suitability of lines to represent forces is obvious enough in ordinary Mechanics, where + has the signification of mere numerical addition; but when we come to Symbolical Mechanics this suitability is no longer a thing to be assumed. A little consideration will show that the question, "Can we assume lines to represent forces generally?" may be stated symbolically as follows, viz. If the lines u and v respectively represent the forces U and V, in magnitude and direction, will $u+v$ also represent U+V in magnitude and direction? if not, the graphical mode of representation becomes inadmissible symbolically. Now, it is clear, by reference to figures 8 and 9, that this question amounts to asking, whether the *Parallelogram of Forces* is true or not? for the peculiar signification of + in Symbolical Geometry makes $u+v$ denote the diagonal of the parallelogram constructed on u and v as sides; whereas the Mechanical signification of + in U+V makes it denote the resultant of U and V.

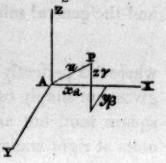
Hence it follows that the *general* representation of forces by lines *assumes* the truth of the Parallelogram of Forces as a necessary condition; and, consequently, any symbolical proof of the Parallelogram of Forces which assumes that lines may be taken generally as representatives of forces, amounts to reasoning in a circle.

My object here, however, in the remarks just made, is to point out the importance of distinctly marking the two significations of the sign +.

(14.) In all cases that I shall be concerned with, *successive* and *simultaneous* addition are virtually equivalent, so far as the representation of directed magnitudes by lines is concerned. As regards the *statical effect of forces*, the Parallelogram of Forces shows this: as regards the *dynamical effect*, the Second Law of Motion (I mean NEWTON'S 2nd Law) does the same. As regards *velocities* and *displacements* the thing is obvious.

(15.) *Directed Units.* I shall call an arrow of a unity of length (whether it represents a traced line, a force, a velocity, or any other kind of directed magnitude) a "*directed unit.*" I shall always reserve the letters α, β, γ to denote a set of three directed units *at right angles to each other.* Hence, if AX, AY, AZ be three rectangular axes to which α, β, γ are respectively parallel; and if x, y, z denote numerically the three coordinates of any point P; $x\alpha$, $y\beta$, $z\gamma$ will be the symbols representing these three coordinates *in magnitude and direction*, inasmuch as $x\alpha$ means x directed units *put together* by *successive* or *geometrical* addition, all in the direction parallel to AX; and so also as regards $y\beta$ and $z\gamma$. Also if u be taken to denote the line AP in magnitude and direction, we have, by *successive* addition,

Fig. 10.



$$u = x\alpha + y\beta + z\gamma.$$

The point P is often called *the point* (xyz), I may therefore speak of it as *the point* (u), inasmuch as u completely defines its position.

If X, Y, Z denote *numerically* three forces parallel to AX, AY, AZ, it is clear that their complete symbolical representatives are $X\alpha$, $Y\beta$, $Z\gamma$. Also, if U denote the resultant of these three forces, we have

$$U = X\alpha + Y\beta + Z\gamma.$$

But here + denotes *simultaneous* addition: we may, however, assuming the truth of the Parallelogram of Forces, regard it as the *successive* +, if we please.

(16.) As just observed, I shall always suppose α, β, γ to be a set of *three rectangular directed units*; I shall suppose the same also as regards α', β', γ'; $\alpha'', \beta'', \gamma''$, &c., using the dashes to denote different sets of directed units; but the three in each set are always assumed to be at right angles to each other, unless the contrary be specified.

In speaking of lines as regards magnitude and direction, I shall always use the word "*direction*" as equivalent to "directed unit;" thus I shall call α the "*direction*" of the line $x\alpha$. The *complete symbol* of a line may be therefore described as *its direction multiplied by its magnitude.*

(17.) If r denote the magnitude, and α' the direction of u, $u = r\alpha'$, and therefore, putting for u its value above, we find

$$\alpha' = \frac{x}{r}\alpha + \frac{y}{r}\beta + \frac{z}{r}\gamma,$$

or

$$\alpha' = a\alpha + b\beta + c\gamma,$$

where a, b and c denote the cosines of the angles which α' makes with α, β and γ respectively.

If α' lies in the plane $(\alpha\beta)$, and if θ denote the angle which α' makes with α, this expression becomes

$$\alpha' = \alpha \cos\theta + \beta \sin\theta.$$

(18.) According to the principles of Symbolical Algebra, we have

$$\beta = -\iota\alpha, \quad \gamma = -\iota\beta, \quad \alpha = -\iota\gamma, \quad (-\iota = \sqrt{-1}).$$

But it is to be remembered that the sign $-\iota$ here does not denote the same identical operation in these three cases; nor is it necessary that it should, any more than the sign $-$. The true state of the case is this, that $(-\iota)\alpha$ is defined by the equation

$$(-\iota)(-\iota)\alpha = -\alpha;$$

and the general solution of this equation is

$$-\iota\alpha = \beta\cos\theta + \gamma\sin\theta,$$

where θ is perfectly arbitrary. Consequently the extraction of the square root of $-$ gives, not simply two values, positive and negative, as in ordinary extractions of the square root, but an infinite number of values, namely the *whole circle* of directed units at right angles to α.

I shall have no occasion to make any use of the sign $-\iota$, or any reference to the connections just given between α, β and γ, except in some future applications of my method, chiefly in Geometry. The statement just made is intended to show what α, β, γ are with reference to the square roots of $-$ (or -1), and to point out distinctly that α, β, γ are *not* supposed to be *square roots of unity*, but merely *direction-units*.

(19.) *Remarkable signification of* $d\alpha$, $d\beta$, $d\gamma$. This signification I pointed out and made use of in a paper read before the Cambridge Philosophical Society (Nov. 1846), and it may be briefly stated here for the purpose of reference in certain applications of the present method. If α and α' be two directed-units at an indefinitely small angle to each other, we have $\alpha' - \alpha = d\alpha$; but $\alpha' - \alpha$ is the line joining the extremities of α and α', and this line is at right angles to α and α' (ultimately), because α and α' are lines of equal length. Hence $d\alpha$ is the expression for an indefinitely small line *at right angles to* α.

Fig. 11.

This signification of $d\alpha$ is one of great importance in Symbolical Geometry and Mechanics: thus for example, if α, β, γ denote direction-units *fixed* in a rigid body, the angular velocities of the rigid body are represented, in magnitude and direction, by

$$\frac{d\alpha}{dt}, \quad \frac{d\beta}{dt}, \quad \frac{d\gamma}{dt},$$

inasmuch as $d\alpha$, $d\beta$, $d\gamma$ represent, in magnitude and direction, the small angles described in the time t by the extremities of these three direction-units. Of course I mean by the word "*angle*," here, the circular arc which measures it. The importance of this signification of $d\alpha$, $d\beta$, $d\gamma$ will be manifest in many parts of what follows.

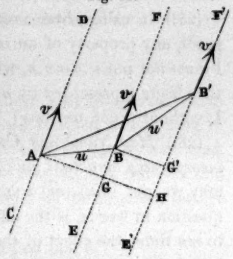
II. Symbolical Representation of the two Effects produced by the Translation of a Directed Magnitude.

(20.) *The Effects symbolized.*—It has been shown that the translation of v *along* u is, generally, of a twofold nature, partly *lateral* and partly *longitudinal*; it is my object to express symbolically the *effects produced*, whether they are geometrical or mechanical effects, by the two kinds of translation. The effect produced by the lateral part of the translation of v along u I shall call *the lateral effect*, and that produced by the longitudinal part, *the longitudinal effect*.

Fig. 12.

(21.) *The two Effects are, each, Distributive Functions of* u *and* v. Let $f(u, v)$ denote the *lateral effect* of the translation of v along u; let u and u' represent the lines AB and BB'; produce the arrows (v) both ways indefinitely to show the *lines of direction* in which v lies in the three parallel positions at A, B, and B'; draw AGH and BG' at right angles to these parallel lines of direction (CD, EF, E'F'). Observe, that v, u and u' are not necessarily in the same plane.

Fig. 13.



Now, as assumed, $f(u, v)$ denotes the effect produced by the shifting of the line CD to the parallel position EF; $f(u', v)$ denotes that by a farther shifting, namely from EF to the parallel position E'F': which two shiftings "*put together*" come to the same thing as one shifting from CD to E'F'. Now since AB' is represented by $u+u'$, the effect of this last-mentioned shifting is denoted by $f(u+u', v)$: we have therefore

$$f(u, v)+f(u', v)=f(u+u', v),$$

that is, the *lateral effect* of the translation of v along u is a *distributive function as regards* u.

In precisely the same way it may be shown, that the *longitudinal effect* is also a distributive function as regards u. For it is manifest that the three translations, viz. that along u, that along u', and that along $u+u'$, amount respectively to GB, G'B', and HB', as regards longitudinal effect, and we have HB'=GB+G'B'. Whence the conclusion is evident.

It remains to show that both effects are distributive functions *as regards* v *also*; and this is immediately obvious: for the translation of v along u "*together with*" that of v' along u, is the same thing as the translation of $v+v'$ along u; and thus, whether f denote the lateral or longitudinal effect, we have

Fig. 14.

$$f(u, v)+f(u, v')=f(u, v+v').$$

Both effects therefore are distributive functions with respect to v as well as u.

(22.) It is important to observe, here, that the $+$ in $u+u'$ denotes *successive*, while that in $v+v'$ denotes *simultaneous addition*.

(23.) *Notation adopted to represent the two effects.* I have stated above the reasons why $u.v$ and $u\times v$ may be appropriated to denote, simply, *any* distributive functions of u and v. I have here shown the existence of two such functions, very important to be "noted" symbolically, and to be distinguished from each other. I shall therefore venture farther to employ the notation $u.v$ *exclusively* for the purpose of representing the *lateral effect* of the translation of v along u, and the notation $u\times v$ *exclusively* to represent the *longitudinal effect*.

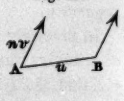
(24.) As regards the *order of the factors*, I shall always suppose that the *second factor* is the translated magnitude, and the *first factor* the line along which it is translated.

(25.) In using these notations I am not warranted to attribute to them, without proof, any property of an ordinary product, except its distributiveness: for example, I must not put $u.v=v.u$, without investigating whether this equation holds as regards the effects represented by $u.v$ and $v.u$. Nor again, if m and n be any numbers, can I, without proof, put $(mu).(nv)=mn(u.v)$. These points I shall now consider.

(26.) *May Numerical Coefficients, occurring in* u.v *or* u×v, *be brought out and incorporated by actual multiplication*?—Supposing m and n to denote pure numbers, may we put $(mu).(nv)=mn(u.v)$, and $(mu)\times(nv)=mn(u\times v)$? Or, to express the question in words, is the effect produced by the translation of nv along mu equivalent to mn times the effect of the translation of v along u? It is very important to bear in mind, as regards this question, that nv means $v+v+v+$ &c. "*put together*" by *simultaneous* addition; while mu means $u+u+u+$ &c. "*put together*" by *successive* addition (see art. 22). Hence it will not be difficult to show that $u.(nv)=n(u.v)$ in virtue of the distributive property; but, that some additional consideration is requisite to determine whether $(mu).v=m(u.v)$.

(27.) *First*, as regards $u.(nv)$. The n v' here have the same origin A, and they are translated simultaneously from A to B. This translation is manifestly the same as if each v were translated separately from A to B: and thus it follows that the translation of nv along u is the same thing as n translations of v along u; or, in symbols,

Fig. 15.

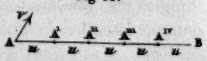


$$u.(nv)=n(u.v), \text{ and } u\times(nv)=n(u\times v).$$

Indeed this is nothing more than a re-assertion of the distributive nature of the translation of v along u, as regards v.

(28.) *Secondly.* In the expression $(mu).v$ the u' have *not* the same origin, but are "*put together*" *successively*, as is represented in fig. 16, making up the line AB (supposing, for a moment, that $m=5$). It is clear, then, that $(5u).v$ means a translation of v from A to B, while 5 $(u.v)$ means five translations of v from A to A′. Hence, before we can decide whether $(mu).v=m(u.v)$, we

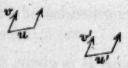
Fig 16.



must determine whether the translations of v from A to A′, from A′ to A″, from A″ to A‴, &c. are equivalent to each other *in effect*; for, if they are, it makes no difference *in effect* whether we repeat the translation of v from A to A′ five times, or simply translate v from A to A′, from A′ to A″, from A″ to A‴, and so on to B. The question then comes to this—Are we to regard translations as equivalent to each other, in effect, when the magnitudes translated and the lines along which they are translated are respectively equivalent to each other, whether the translations take place *in the same part of space or not*?

(29.) *Fundamental Assumption.*—I am thus led to make the following *Assumption* the basis of my proposed method; viz.—*That parallel and equal translations of parallel and equal magnitudes are equivalent to each other both as regards the lateral and longitudinal effects*; or, symbolically, if u' and v' be respectively parallel and equal to u and v, then

Fig. 17.



$$u'.v'=u.v, \text{ and } u'\times v'=u\times v.$$

This assumption holds true, manifestly, in each of the three *suggesting cases* from which I have taken my start (see art. 2), and I am therefore justified in adopting it, *with the understanding*, of course, that it be shown to hold true, or tacitly admitted, in all cases to which the notation may be applied; or else, should the occasion require it, be abandoned, and, with it, the property expressed by the equation $(mu).v=m(u.v)$.

(30.) Returning to fig. 16, we have, by the Assumption just made,

$$AA'.v=A'A''.v=A''A'''.v=\text{ \&c. \&c.};$$
$$\therefore m(u.v)=m(AA'.v)=AA'.v+A'A''.v+A''A'''.v+\text{ \&c.}$$
$$=(AA'+A'A''+A''A'''+\text{ \&c.}).v$$
$$=(mu).v.$$

And generally, by what has been proved, we have

$$(mu).(nv)=n\{(mu).v\}=mn(u.v);$$

and, similarly, $$(mu)\times(nv)=mn(u\times v).$$

It appears thus that *numerical coefficients*, occurring in the symbolic forms $u.v$ and $u\times v$, may always be *brought out* and incorporated by actual multiplication*.

(31.) *May the order of the factors* u *and* v *in the symbolic forms* u.v *and* u×v *be changed, or not?—First*, as regards $u.v$, may we put $u.v=v.u$? Here I may repeat that the second factor always denotes the translated magnitude, or rather, the representative arrow. Thus the question is—as regards lateral effect, is the translation of the magnitude represented by the *arrow* v along the *line* u equivalent to the translation of that represented by the *arrow* u along the *line* v. This is easily decided as follows.

(32.) It is clear that the *lateral effect* of the translation of a magnitude *in its own*

* It may be shown, in the usual way, that this is true also when m and n are *fractional* or *negative* numbers.

direction is zero: it follows therefore that $u.u=0$, $v.v=0$; also that

$$(u+v).(u+v)=0,$$
$$\therefore u.u+u.v+v.u+v.v=0,$$
$$\therefore u.v+v.u=0,$$
$$\text{or } v.u=-u.v.$$

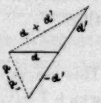
It appears then that $u.v$ and $v.u$ are *equivalent as regards magnitude* but *opposite in sign*.

(33.) *Secondly*, as regards $u\times v$, may we put $v\times u=u\times v$? This question is determined by observing that the *longitudinal effect* of the translation of a magnitude *at right angles to its direction is zero*, as follows.

Let $u=m\alpha$, $v=m'\alpha'$, α and α' being the *directions* (directed units), and m and m' the magnitudes of u and v. Then, by article 30,

$$u\times v=mn(\alpha\times\alpha') \text{ and } v\times u=mn\ (\alpha'.\alpha).$$

Fig. 18.



Now it is clear, from figure 18, that $\alpha+\alpha'$ and $\alpha-\alpha'$ are lines at right angles to each other; therefore

$$(\alpha+\alpha')\times(\alpha-\alpha')=0=(\alpha-\alpha')\times(\alpha+\alpha');$$

therefore, omitting common terms, we find

$$-\alpha\times\alpha'+\alpha'\times\alpha=\alpha\times\alpha'-\alpha'\times\alpha,$$
$$\therefore \alpha\times\alpha'=\alpha'\times\alpha.$$

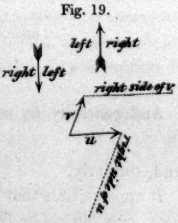
And thus it follows that

$$u\times v=v\times u.$$

It appears then that $u\times v$ and $v\times u$ are *equivalent as regards both magnitude and sign*.

(34.) Thus $u.v$ is *commutative with change of sign*, while $u\times v$ is *simply commutative*. The reason of the change of sign in the former may be easily interpreted as follows. An arrow has two distinct *sides*, which, for the sake of fixing ideas, I may call *right* and *left*, and which may be defined by supposing that I stand *on* the plane of the paper *looking in the direction of the arrow*. Now, referring to fig. 19, it is clear that the translation of v along u is *laterally* a motion *to the right side*, while that of u along v is *to the left*, the translated magnitude in both cases being that with reference to which I speak of right and left. Thus the meaning of the equation $v.u=-u.v$ is obvious.

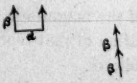
Fig. 19.



III. Measurement and Summation of Translations.

(35.) *Units of Translation.*—I shall take the translation of a unit *along a perpendicular unit* to be a *unit of lateral translation*; and the translation of a unit *along itself* to be a *unit of longitudinal translation*. Thus (see art. 16) $\alpha.\beta$, $\beta.\gamma$, $\alpha'.\beta'$, &c. are units of lateral translation; and $\alpha\times\alpha$, $\beta\times\beta$, $\alpha'\times\alpha'$, &c. are units of longitudinal translation.

Fig. 20.



(36.) *All Units of Longitudinal Translation are equivalent to each other.* For let α and α' denote any two directed units whatever; then, as above,

$$(\alpha+\alpha')\times(\alpha-\alpha')=0;$$

wherefore, since $\alpha\times\alpha'=\alpha'\times\alpha$, we have

$$\alpha\times\alpha=\alpha'\times\alpha'.$$

In illustration of this result the third *suggesting instance*, that of *Mechanical Work*, may be quoted; inasmuch as work is the effect of longitudinal translation, and all units of work are equivalent to each other, no matter in what directions the working forces act.

(37.) *All Units of Lateral Translation, in the same or in parallel planes, are equivalent to each other.*—Let α, β, α', β' lie in the same plane, and let θ denote the angle which α' makes with α, and therefore that also which β' makes with β (art. 16): then (art. 17)

$$\alpha'=\alpha\cos\theta+\beta\sin\theta$$

$$\beta'=\alpha\cos\left(\theta+\tfrac{\pi}{2}\right)+\beta\sin\left(\theta+\tfrac{\pi}{2}\right);$$

$\therefore$, observing that $\alpha.\alpha=\beta.\beta=0$, and $\alpha.\beta=-\beta.\alpha$, we have

$$\alpha'.\beta'=\alpha.\alpha(\cos^2\theta+\sin^2\theta)=\alpha.\alpha.$$

Hence all units of lateral translation in the same, or in parallel planes, are equivalent to each other.

In illustration of this result, the second *suggesting instance*, that of a *Couple*, may be quoted; for all unit-couples in the same or in parallel planes are equivalent to each other.

(38.) *Directrix.*—Hence, in expressing a unit of lateral translation, it is only necessary to specify a plane parallel to that in which the translation takes place; or, what is better and immediately suggested by the theory of couples, it is only necessary to specify *a line at right angles to the plane of translation.* Such a line I shall, however, designate by the word "*directrix*," not *axis*; because there is no idea of rotation involved in the present theory, translation being a kind of motion essentially different from rotation. I shall assume γ to be the directrix of $\alpha.\beta$ and of all units of lateral translation in planes at right angles to γ; and, generally, I shall define the directrix of any unit of lateral translation to be a directed unit at right angles to the plane of that translation.

(39.) But, since $\alpha.\beta=-\beta.\alpha$, it is necessary to distinguish positive from negative translations; and this may be done by giving an appropriate sign to the directrix. I shall therefore assume generally, that $m\gamma$ is the directrix of $m\,\alpha.\beta$, m being any number positive or negative. Thus $-\gamma$ will be the directrix of $-\alpha.\beta$, that is, of $\beta.\alpha$. And, hence, I may adopt the following criterion of

Fig. 21.

sign. I shall suppose myself standing at right angles to the plane of translation in such a position that the translated magnitude points to the right, while I face the direction in which the translation takes place; and then I shall take an arrow pointing *from foot to head* as the directrix. According to this criterion the letters written underneath the following translations are their respective directrices; viz.

$\alpha.\beta$	$\beta.\gamma$	$\gamma.\alpha$	$\beta.\alpha$	$\gamma.\beta$	$\alpha.\gamma$
γ	α	β	$-\gamma$	$-\alpha$	$-\beta$

(40.) *Measurement of a Translation.*—Let $u.v$, or $u\times v$, be the translation, α the direction of u, $(\alpha\beta)$ the plane of $u.v$, θ the angle which v makes with u, m and n the magnitudes of u and v; then

$$u=m\alpha, \quad v=n(\alpha\cos\theta+\beta\sin\theta);$$
$$\therefore\ u.v=mn\sin\theta(\alpha.\beta), \quad (\text{since } \alpha.\alpha=0).$$

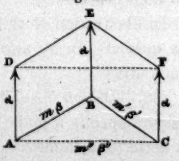
Hence there are $mn\sin\theta$ units in the translation $u.v$: $mn\sin\theta$, therefore, is the numerical magnitude of $u.v$, and its directrix is $(mn\sin\theta)\gamma$.

Again, $u\times v=mn\cos\theta(\alpha\times\alpha)$, (since $\alpha\times\beta=0$). Hence the numerical magnitude of $u\times v$ is $mn\cos\theta$.

(41.) It is worth remarking that $mn\sin\theta$, the numerical magnitude of $u.v$, is the area of the parallelogram completed on u and v as sides.

(42.) *The Directrix of the Sum of two translations is the Sum of their Directrices.*—Let the two translations be $m\alpha.\beta$ and $m'\alpha'.\beta'$, and their directrices, of course, $m\gamma$ and $m'\gamma'$; let EB be the intersection of the two planes of these translations, and take AB$=m$ and BC$=m'$, AB and BC being drawn at right angles to BE, AB in the plane of $\alpha.\beta$, and BC in the plane of $\alpha'.\beta'$.

Fig. 22.



Now, since all units of lateral translation in the same plane are equivalent to each other, I may turn α and β, α' and β' about in their respective planes, until both α and α' coincide with BE; in which case β and β' will coincide with AB and BC respectively; then AB will become $m\beta$, and BC $m'\beta'$. Let the third side of the triangle ABC be $m''\beta''$ as shown in the figure. Then the sum of the two translations is

$$m\alpha.\beta+m'\alpha'.\beta',$$

which, since $\alpha'=\alpha$, becomes

$$\alpha.(m\beta+m'\beta');$$

and this, since $m\beta+m'\beta'=m''\beta''$, becomes

$$m''\alpha.\beta''.$$

Let $m''\gamma''$ be the directrix of this: then it is clear that γ, γ' and γ'', being each at right angles to α, lie in the plane of the triangle ABC, and consequently $m\gamma$, $m'\gamma'$ and $m''\gamma''$ are the three sides of the triangle ABC, supposing it to be turned round in its plane through 90°. It follows therefore that

$$m''\gamma''=m\gamma+m'\gamma',$$

that is, the directrix of the sum of the two translations is the sum of their directrices.

(43.) Generally, it is manifest from this result, that the *Directrix of the Sum* of any number of Translations is the *Sum of their Directrices.*

It is not necessary to point out the importance of this rule as regards the *summation* of translations, nor its identity with the well-known rule in the *Theory of Couples.*

(44.) For the sake of convenience it will be worth while to employ some abbreviated mode of specifying the directrix of any translation; for this purpose I shall adopt the following notation, which, it will be found, will answer all purposes, and at the same time very distinctly mark that property in which the peculiar relation between a translation and its directrix consists. The property I allude to is the theorem just proved in article 42.

I shall employ the letter D to stand as an abbreviation for the words "*directrix of*," and thus $\mathrm{D}(u.v)$ will mean *the directrix of the translation* u.v. It will be borne in mind, then, that $\mathrm{D}(u.v)$ denotes a line at right angles to the plane of $u.v$, and containing as many units of length as there are units of area in the parallelogram constructed on u and v.

(45.) *Distributiveness of the Operation thus represented.*—It is most important to notice the *distributive* nature of the symbol D. By art. 42, we have immediately

$$\mathrm{D}(u.v)+\mathrm{D}(u'.v')=\mathrm{D}(u.v+u'.v');$$

whence, D denotes a distributive function.

(46.) *Consequences hence resulting.*—From the equation in art. 45 it follows, that, if m denote any numerical coefficient, positive or negative,

$$\mathrm{D}(mu.v)=m\mathrm{D}(u.v).$$

Again, since

$$\mathrm{D}(u'.v')-\mathrm{D}(u.v)=\mathrm{D}(u'.v'-u.v),$$

we have, passing to limits,

$$d\big(\mathrm{D}(u.v)\big)=\mathrm{D}\big(d(u.v)\big);$$

whence also,

$$f\big(\mathrm{D}(u.v)\big)=\mathrm{D}f(u.v).$$

In short, in all operations in which differentiation and integration are concerned, D is to be regarded as if it were an ordinary constant coefficient.

Again, if we have an equation of the form

$$u.v+u'.v'+u''.v''+\&c.=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

there result from it

$$\mathrm{D}(u.v)+\mathrm{D}(u'.v')+\mathrm{D}(u''.v'')+\&c.=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

And, conversely, (2.) gives (1.).

(47.) *Inverse of* D.—If w be the directrix of $u.v$, and therefore $w=\mathrm{D}(u.v)$; I may of course, according to the true force of the index (-1), assert, that $u.v=\mathrm{D}^{-1}w$. Thus $\mathrm{D}^{-1}w$ comes to be an abbreviation for the words—"*the translation whose directrix is w.*"

(48.) It may be well to observe that the following relations result from what has been said, viz.

$$\mathrm{D}(\alpha.\beta)=\gamma \qquad \mathrm{D}(\beta.\gamma)=\alpha \qquad \mathrm{D}(\gamma.\alpha)=\beta$$
$$\mathrm{D}(\beta.\alpha)=-\gamma \qquad \mathrm{D}(\gamma.\beta)=-\alpha \qquad \mathrm{D}(\alpha.\gamma)=-\beta$$

whence

$$\alpha.\beta=\mathrm{D}^{-1}\gamma \qquad \beta.\gamma=\mathrm{D}^{-1}\alpha \qquad \gamma.\alpha=\mathrm{D}^{-1}\beta$$
$$\&c. \qquad \&c. \qquad \&c.$$

(49.) *Symbol for Units of Longitudinal Translation.*—It has been shown that these units are all equal to each other; a *unit of longitudinal translation* is therefore an *absolute constant*. No distinctive symbol is necessary, therefore, to represent these units, and it will be allowable to employ the common unit (1) for the purpose; just in the same way that we denote all units, whether they be linear, superficial, cubical, mechanical, by this common symbol. I shall therefore always represent a unit of longitudinal translation by 1; and thus put

$$\alpha\times\alpha=1, \qquad \beta\times\beta=1, \qquad \gamma\times\gamma=1;$$

and generally, if m and n denote the magnitudes of u and v, we have (art. 41)

$$u\times v=mn\cos\theta,$$

θ being the angle made by u and v.

(50.) Hence if α and α' be any two "*directions*," we have

$$\alpha\times\alpha'=\text{cosine of angle made by } \alpha \text{ and } \alpha'.$$

To this may be added

$$\text{magnitude of } \alpha.\alpha'=\text{sine of same angle.}$$

(51.) *Projections represented by the lateral and longitudinal translation-products.*—It is clear from the principles just established, that, if α, β, γ denote the directions of three coordinate axes, and v any line, the *projections* of v on the three axes are, numerically,

$$\alpha\times v, \qquad \beta\times v, \qquad \gamma\times v.$$

Again, since $u\times u$ is the square of the magnitude of the line u, the projection of v on u is, numerically,

$$\frac{u\times v}{\sqrt{u\times u}},$$

which, putting for u and v the values $x\alpha+y\beta+z\gamma$, $x'\alpha+y'\beta+z'\gamma$, becomes by *longitudinal multiplication*,

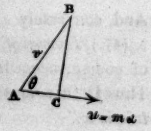
$$\frac{xx'+yy'+zz'}{\sqrt{x^2+y^2+z^2}}.$$

I may use the terms "*lateral* and *longitudinal multiplication*" to designate the operations denoted by $u.v$ and $u\times v$; for the word "multiplication" has quite lost its original and proper signification even in ordinary algebra.

(52.) If AB represent v, AC the projection of v on u; the magnitude and direction of u being m and α; then

$$\mathrm{CB}=\mathrm{AB}-\mathrm{AC}=v-(\alpha\times v)\alpha.$$

Fig. 23.



I think CB might be advantageously called the *Complement of the Projection* of v on u, for CB added to AC makes up or *completes* v; and thus, employing the usual abbreviation, we may call CB the *coprojection* of v on u.

Hence the operation $(\alpha(\alpha\times)$ performed on v gives the *projection* of v on u, and the operation $(1-(\alpha\times\alpha)$ the *coprojection*; where α denotes the "*direction*" of u, *i. e.*

$$\frac{u}{m} \text{ or } \frac{u}{\sqrt{u\times u}}.$$

(53.) Since $\alpha.v=\alpha.(AC+CB)=\alpha.CB$, it is clear that the symbol $\alpha.v$ denotes, in magnitude, the coprojection of v on α; and it also represents the plane of projection. But $D\alpha.v$ is a better symbol to use; for its magnitude is the same as that of $\alpha.v$, and, in direction, it denotes a line at right angles to the plane of projection.

(54.) *Repetition of the Operation* $(D\alpha.)$. This operation is often to be performed twice in investigations, and, on this account, the following relation is important, viz.

$$(D\alpha.)^2\alpha'=(\alpha\times\alpha')\alpha-\alpha'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Of course $(D\alpha.)^2\alpha'$ means $D\alpha.(D\alpha.\alpha')$.

To prove this, let β be chosen so as to lie in the plane $(\alpha\alpha')$, and let θ denote the angle which α and α' make with each other; then (by art. 17)

$$\alpha'=\alpha\cos\theta+\beta\sin\theta$$
$$D\alpha.\alpha'=\gamma\sin\theta \text{ (art. 48)}$$
$$D\alpha.(D\alpha.\alpha')=-\beta\cos\theta$$
$$=\alpha\cos\theta-\alpha'$$

or $$(D\alpha.)^2\alpha=(\alpha\times\alpha')\alpha-\alpha' \text{ (art. 50).}$$

If $u=m\alpha$ $v=n\alpha'$, we have

$$(Du.)^2v=m^2n(D\alpha.)^2\alpha'$$
$$=(m\alpha\times n\alpha')m\alpha-(m\alpha\times m\alpha)n\alpha',$$

or $$(Du.)^2v=(u\times v)u-(u\times u)v; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

or, if $m=1$, $$(D\alpha.)^2v=(\alpha\times v)\alpha-v. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

(55.) *Relation of the Operation* $(D\alpha.)$ *to the operation* $\sqrt{-1}$ *or* $(-)^{\frac{1}{2}}$. The definition of the index $\frac{1}{2}$ in relation to operations is this. If Ω and Ω_1 be symbols of operation, such that Ω performed twice on a quantity gives the same result as Ω_1 once performed, then Ω is denoted by $\Omega_1^{\frac{1}{2}}$. Now, if we suppose that v is at right angles to α, and therefore $\alpha\times v=0$, the equation (3.) gives (what indeed is otherwise more easily shown from art. 48)

$$(D\alpha.)^2v=-v,$$

wherefore

$$D\alpha.=(-)^{\frac{1}{2}}.$$

In this case α is any unit line whatever at right angles to v, and therefore, in Solid Geometry, $(-)^{\frac{1}{2}}$ or $\sqrt{-1}$ has not two (as in Plane Geometry) but an infinite number of values.

(56). It is clear from the relation

$$(D\alpha.)^2v=-v,$$

that $(D\alpha.)$ has all the properties of the sign $\sqrt{-1}$, *provided it be performed on lines at right angles to* α. But $(D\alpha.)$ is a far better sign for actual use in Solid Geometry than

$\sqrt{-1}$, because the latter is indefinite, not distinguishing what particular root of $-$ is meant; but the former is perfectly definite, inasmuch as it indicates one particular root of $-$, namely, that root which denotes rotation through 90° about α as axis*.

* Sir W. Hamilton, in his System of Symbolic Geometry and Quaternions, which may be truly described as one of the most profound and beautiful theories in the whole range of abstract science, assumes the letters i, j, k to denote particular values of $\sqrt{-1}$. In a certain limited sense, the symbols $(D\alpha.)$, $(D\beta.)$, $(D\gamma.)$ are equivalent to these; for i, j, k denote rotation through 90° about the axes α, β, γ; and thus we have

$$\gamma = i\beta, \quad \alpha = j\gamma, \quad \beta = k\alpha;$$

and, therefore, since

$$\gamma = D\alpha.\beta, \quad \alpha = D\beta.\gamma, \quad \beta = D\gamma.\alpha,$$

it is clear that i, j, k, and $(D\alpha.)$, $(D\beta.)$, $(D\gamma.)$, so far, denote the same operations.

But the operations are different *in general*; for $i^2 = -1$ *always*, but $(D\alpha.)^2$, as has been shown above, is *not* equivalent to -1, *except* when performed on lines at right angles to α.

There is one difficulty, I confess, I cannot get over in Sir W. Hamilton's Theory, no doubt from some misconception on my part, or from taking too narrow a view of the meaning of the sign $\sqrt{-1}$. The difficulty I allude to consists in this. Sir W. Hamilton assumes i, j, k not only to be particular values of $\sqrt{-1}$, but also *absolute directions* (i. e. *units of direction*): in short he uses i, j, k in the same sense as α, β, γ above, and in the same sense *also* as $(D\alpha.)$, $(D\beta.)$, $(D\gamma.)$. Now my difficulty arises from my not being able to see how particular values of $\sqrt{-1}$ can denote anything (geometrically) but *change* of direction, or to perceive, that they can be used with propriety as symbols of those rectangular units of direction.

However this may be, it is important to explain the fact, that, in the results to which I have been led by the conception of translation, there are no general relations corresponding to

$$i^2 = -1, \quad j^2 = -1, \quad k^2 = -1.$$

In my method, α, β, γ are *simple units of direction* and *nothing more*; and instead of the relations just put down, I have been led, by the conception of translation, to the following, viz.—

$$\alpha.\alpha = 0, \quad \beta.\beta = 0, \quad \gamma.\gamma = 0$$
$$\alpha \times \alpha = 1, \quad \beta \times \beta = 1, \quad \gamma \times \gamma = 1;$$

though, as regards the last three relations, all that I have a right to assert as a matter of necessity is, that

$$\alpha \times \alpha = \beta \times \beta = \gamma \times \gamma.$$

Also, I find

$$(D\alpha.)^2 = -1, \quad (D\beta.)^2 = -1, \quad (D\gamma.)^2 = -1,$$

but only when performed on lines at right angles to α, β, γ respectively.

I may observe that if uv denote the product of u and v according to Sir W. Hamilton's Theory, it may be thus expressed in terms of my *translation-products*; viz.—

$$uv = -u \times v + Du.v.$$

Hence, since

$$Du.v = -Dv.u, \text{ and } u \times v = v \times u,$$
$$vu = -u \times v - Du.v.$$

Wherefore

$$u \times v = -\frac{1}{2}(uv + vu)$$
$$Du.v = \frac{1}{2}(uv - vu).$$

I may also observe, that, according to my method, I might put

$$uv = u \times v + u.v,$$

supposing that uv denotes the complete product of the translation of u along v, *including both* the lateral and longitudinal effects. But I cannot make any nearer approximation to the equation

$$uv = -u \times v + Du.v;$$

nor can I see that the conception of translation furnishes any interpretation of the $-$ before $u \times v$.

If, in any way, I could show that -1 was the proper value for a unit of longitudinal translation, I should have

$$\alpha\alpha = \alpha \times \alpha + \alpha.\alpha = -1.$$

(57.) The lateral translation-product is therefore well adapted to denote *rotation*. Thus (always supposing that v is at right angles to α), the symbol of a line *making an angle* θ *with* v, *and in a plane at right angles to* α, is

$$\epsilon^{\theta D\alpha}v,$$

which in fact is the same thing as

$$\{\cos\theta+(D\alpha.)\sin\theta\}v,$$

for $\cos\theta$ and $\sin\theta$ represent

$$1-\frac{\theta^2}{1.2}+\frac{\theta^4}{1.2.3.4},$$

and

$$\frac{\theta}{1}-\frac{\theta^3}{1.2.3}+\frac{\theta^5}{1.2.3.4.5},$$

and $\epsilon^{\theta D\alpha.}$ is the symbolic form for expressing

$$1+\frac{(\theta D\alpha.)}{1}+\frac{(\theta D\alpha.)^2}{1.2}+;$$

whence, since $(D\alpha.)^2=-(D\alpha.)^4=+$ &c., the symbolic equivalence of the forms

$$\epsilon^{\theta D\alpha.} \text{ and } \cos\theta+(D\alpha.)\sin\theta$$

is manifest. But the quantity operated upon must necessarily be at right angles to α; for, otherwise, $(D\alpha.)^2$ is not equivalent to $-$, as appears from art. 54, equation (3.).

(58.) *Concluding Remarks.*—I have now said enough, I think, to explain the nature of the proposed symbolization, and the general rules which regulate the application of the two *translation-products*. I have based the whole theory *simply and exclusively* on the conception of translation, taking my clue from the three suggesting instances, the *parallelogram*, the *couple*, and *work*. As regards the lateral effect of translation, the theory is nothing but a general development of our *geometrical* notion of multiplication; for what is a rectangle, ABCD, *considered apart from arithmetical measure*, but the effect or *product* of the translation of AB along AD? and this we represent by writing AD before or after AB. But this method of representation is clearly incomplete when we put for AB and AD their numerical representations; and why? because the special superficial unit then is omitted. For, suppose AB=3, AD=4; then, if we say that the rectangle ABCD is the product of 3 and 4, or 12, we mean, 12 *superficial units*. Now, by omitting the superficial unit in our representation, we leave out all conception of *the plane* in which the rectangle lies. All that I have done above is *to restore the superficial unit*, and determine its proper representative symbol. As regards the *longitudinal-effect* (suggested by the conception of work), it appears that all units are absolutely equivalent, and therefore may be all confounded in the common symbol of unity.

Fig. 24.

I now proceed to give Applications of the Symbolic forms* $u.v$ and $u\times v$, &c.

* I may refer here to an imperfect attempt I made in a paper read before the Cambridge Philosophical Society (Nov. 1846) to base the symbolic form $Du.v$ on the conception of perpendicularity in art. 19 above.

PART II.

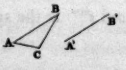
APPLICATIONS OF THE SYMBOLIC FORMS.

I. Geometrical Applications of the Symbolic Forms.

The geometrical applications which may be made of the principles and notation just explained are of great variety and importance; but, as I am anxious to dwell chiefly on physical applications, I shall only touch on this part of the subject.

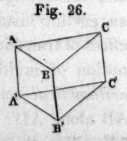
(59.) *Surface symbolically considered.*—In Symbolic Geometry the symbols of lines represent, not merely length but also direction. A line is supposed to be the geometrical effect of the motion of a tracing-point, and the equivalence of lines is considered altogether and exclusively with reference to the change which they represent in the position of the tracing-point. We consider that AB is equivalent to A′B′, when the two lines are *of equal length, parallel, and traced the same way*; for then they represent equivalent changes of position of their respective tracing-points. It is usual to employ the notation AB to denote the line AB *traced from* A *to* B, and BA to denote the same line traced from B to A; and thus AB=A′B′, but BA=−A′B′. It is clear then that equivalent lines must be, not only of equal length and parallel, but also must be traced *the same way*. Again, the equivalence being considered only with reference to the tracing-point's change of position, AC+CB is equivalent to A′B′.

Fig. 25.



Now, following this analogy, and regarding surface as the effect of the motion of a *tracing-line*, just as a line is the effect of the motion of a *tracing-point*, we may employ symbols to denote surfaces, not merely as regards their numerical area, but also with reference to the manner in which they are generated by their tracing-lines; and we may also define the equivalence of surfaces altogether and exclusively with reference to the change which they represent in the position of the tracing-line. Thus, if ABA′B′, BCB′C′, ACA′C′ be three parallelograms, and if we conceive them to be generated, respectively, by the *translations* of A′A along A′B′, B′B along B′C′, and A′A along A′C′; it is clear that

Fig. 26.



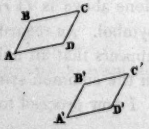
(ABA′B′)+(BCB′C′) is equivalent to (ACA′C′),

just in the same sense that

(AB+BC) is equivalent to (AC).

Again, if ABCD and A′B′C′D′ be two parallelograms, AB and AD being respectively parallel and equal to A′B′ and A′D′, we may consider these parallelograms as equivalent to each other, in the same sense exactly that the lines AB and A′B′ are regarded as equivalent. Only, just as it is necessary for the equivalence of AB and A′B′, that they should be traced *the same way* by their respective *tracing-points*, so it is necessary to the equivalence of ABCD and A′B′C′D′, that they should be traced *the same way* by their respective *tracing-lines*. Now here it is to be observed that a tracing-point is devoid of two important properties which

Fig. 27.



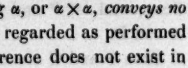
a tracing-line possesses; I mean, *direction* and *two-sidedness* (if I may so speak). An arrow lying on* a plane, not only points in a particular direction, but has *two distinct sides*, right and left (see art. 34). Hence when we speak of lines described *the same way* by their tracing-points, and parallelograms described *the same way* by their tracing-lines, the expression "*same way*" includes much more in the latter than in the former case. For example, the parallelograms generated by the translation of AB along AD, and of DC along DA, though coincident, are described *opposite ways*. Again, the parallelograms generated by the translation AB along AD, and of AD along AB, though coincident, are described opposite ways; for the former is described by a *right-side* motion, and the latter by a *left-side* (see art. 34).

(60.) *Definition of Surface by reference to Lateral Translation.*—It appears to me that the simplest way of including the considerations just alluded to in the general conception of surface requisite in Symbolical Geometry, is to define Surface by reference to the Lateral Effect of the Translation of one line along another. The *Fundamental Assumption* (article 29) is justified in this case, as appears from the remarks just made, and the peculiar relation $u.v=-v.u$ is naturally interpreted (see art. 34). I shall therefore define *Surface* in *Symbolic Geometry* to be *the Lateral Effect of the Translation of one line along another*. By "*Effect*" here I mean simple *Geometrical* effect, *i. e. change of position in space*. Also I only speak of *lateral effect*; because all notion of *longitudinal* effect is excluded by our ordinary conception of surface, and we may assume that no shifting which a line undergoes *in its own direction* can generate *surface*.

(61.) *Longitudinal Effect considered geometrically.*—But though the shifting of a line in its own direction generates no surface, it produces alteration of position; and hence it constitutes an important conception. The only difficulty, in considering the longitudinal effect *geometrically*, consists in this—How is it that all units of longitudinal translation are equivalent (see arts. 36, 37), while those of lateral translation are not, and on what principle can this difference be interpreted? The answer appears to be this: that $\alpha.\beta$ denotes the effect of translating the unit β along the perpendicular unit α; that this operation *conveys the conception of a particular plane*, and we must think of $\alpha.\beta$, $\beta.\gamma$ as different operations because they are performed in different planes. On the contrary, the translation of α along α, or $\alpha\times\alpha$, *conveys no conception of a particular plane*; in fact $\alpha\times\alpha$ and $\beta\times\beta$ may be regarded as performed in the same plane. Thus, that which before made the difference does not exist in this case.

Generally, $u.v=u'.v'$, when the two parallelograms generated are equal in magnitude, and lie in the parallel planes; but $u\times u$ and $u'\times u'$ may be always considered as lying in the same plane; and consequently difference of magnitude only remains to constitute a difference between $u\times v$ and $u'\times v'$.

Fig. 28.



* Not *in* but *on* a plane, that is, on one particular side of the plane; *e. g.* on the *upper side* of this sheet of paper.

Again, as regards sign, there are *four* varieties of the form $u.v$, namely $u.v$, $(-u).(-v)$, both $+^{m}$, and $(-u).v$, $u.(-v)$, both $-^{m}$. But there are only *two* varieties of the form $u \times v$ when v becomes identical with u; inasmuch as v must be the same as u *in sign as well as in magnitude*, and consequently the notation does not admit of the variations $(-u) \times u$ and $u \times (-u)$. Now the other remaining variations are $u \times u$ and $(-u) \times (-u)$, and these have both the same sign.

Hence (supposing that u and v are units), since units of lateral translation may differ from each other in two particulars *only*, namely, *sign* and *plane of translation*, and since units of longitudinal translation are incapable of differing in these particulars, we may see the interpretation of the result in art. 37, and its perfect consistency with that in art. 36.

(70.) *Symbol of a line drawn from a given Point.*—If we assume that simple letters, such as u, v, w always denote lines of particular lengths and drawn in particular directions, *but all starting from the Origin of Coordinates* O; then the proper symbol for denoting a line v *drawn from the point* P, OP being u, will be

$$v + u.v + u \times v;$$

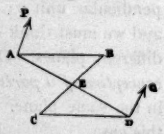
Fig. 29.

for v denotes the line v drawn from O, and $u.v + u \times v$ the lateral and longitudinal effects of translating it from O to P, which effects, as above stated, have reference only to the change of position of P. I shall reserve the consideration of this symbolization for a future occasion, as a striking instance of the same thing will be given in the next section.

II. Statical Application of the Symbolic Forms.

(71.) *Equivalence of Parallel and Equal Translations.*—As a necessary preliminary the *Fundamental Assumption* in art. 29 must be justified. To do this it is only necessary to bear in mind that all statical problems are reducible to the case of balancing forces acting *on the same rigid body*. Now let AB and CD be any two parallel and equal lines in the *same rigid body*; join A and D, C and B; the intersection E bisecting the two joining lines; and let P and Q be two equal parallel forces acting at A and D.

Fig. 30.



Then P and Q are equivalent to P+Q at E, and P+Q at E is equivalent to P at B and Q at C. We may therefore translate P from A to B, provided we, at the same time, translate Q from D to C. Whence it follows that the translation of Q from C to D must be equivalent to the translation of P from A to B. Thus the fundamental assumption is justified.

(72.) *Representation of Forces by Lines.*—It will be remembered that the general symbolic representation of forces by lines assumes the truth of the Parallelogram of Forces. As a matter of curiosity it may be asked, is it possible to apply this Symbolization of Translation to prove the Parallelogram of forces, *without assuming that*

forces are represented by lines? It may be done very simply, and I give the proof here as an example of the application of the method.

It may be seen, on referring to art. 21 and the following articles, that there is no assumption whatever of the possibility of representing forces by lines generally. The arrow representing the translated magnitude is used merely as a *conventional symbol*, just as a letter in algebra. I therefore may apply the notation $u.v$ to the present question, provided I consider v to be the symbol of a force, and not of a line representing that force. I must not however employ the reasoning in art. 32, for that distinctly assumes the point in question. The following then is the proof I shall give.

(73.) *Parallelogram of Forces.*—Let A, B, C denote *any* three units of force, and a, b, c units of length (directed units) parallel respectively to A, B, C; let X, Y, Z, x, y, z be pure numbers; suppose that the forces XA, YB, ZC balance each other, and that xa, yb, zc are the three sides of a triangle formed by lines drawn parallel to the forces. Then by *successive* addition we have

Fig. 31.

$$xa+yb+zc=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

and by *simultaneous* addition

$$XA+YB+ZC=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Hence, from (1.) and (2.), we have

$$(-zc).(-ZC)=(xa+yb).(XA+YB);$$

or, observing that (since no lateral effect is produced by translating a magnitude in its own direction) $c.C$, $a.A$, $b.B$ are each zero, we have

$$0=xY(a.B)+yX(b.A). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

Now, without altering the directions of the units A, B, a, b, let us put $X=Y$; in which case it is self-evident, that ZC must become equally inclined to XA and YB, and therefore zc must make equal angles with xa and yb, which gives $x=y$. Thus (3.) becomes

$$a.B+b.A=0, \text{ or } b.A=-a.B.$$

Wherefore, restoring the inequality of X and Y, we find from (3.),

$$(xY-yX)a.B=0, \text{ or } xY-yX=0.$$

And similarly, we may show that

$$yZ-zY=0$$
$$zX-xZ=0;$$

whence

$$X : Y : Z :: x : y : z.$$

And this is, virtually, the Parallelogram of Forces.

Thus it appears that the notation $u.v$ is capable of affording a simple proof of the great fundamental theorem of Statics; this application of the method is given, however, as I stated above, merely to show by example what can be done in this way. I may observe that the whole of the proof here given depends simply upon *two things*, the distributiveness of $u.v$, and the fact that numerical coefficients may be brought out

and incorporated by actual multiplication. I now proceed to give, generally, the mode of applying the notation to determine the conditions of equilibrium of a rigid body, or a system of particles.

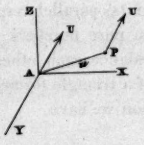
(74.) *Remarkable symbolization of a Force acting at a Specified Point of a Rigid Body.* One of the most remarkable symbolizations which my proposed method leads to appears to me to be the following.

Let U denote a force (in magnitude and direction) *acting at any assumed origin* A, then

$$U+u.U$$

will *completely* denote *the same force supposed to act at the point u*, that is, at the point (P) whose distance from the origin is, symbolically, u. For if we apply the force at the origin A and then translate it to P, it will be the same thing as if we applied the force directly at P. Thus

Fig. 32.



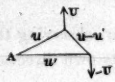
effect of force at P=effect at A (or U)

+effect of translation from A to P.

But, the lateral effect only of the translation need be considered, for the longitudinal effect is zero, inasmuch as we may suppose a force to act at any point of its line of direction on a rigid body. Hence the effect of the force acting at P is symbolically represented by the force U+ lateral effect of translation, or $U+u.U$.

(75.) *Symbol of a Couple.*—Let the couple consist of two forces, U at the point u, and $-U$ at the point u': then these two forces are completely represented, as regards their effect on the rigid body, by the expression

Fig. 33.



$$(U+u.U)+\big(-U+u'.(-U)\big)$$

or

$$(u-u').U,$$

which is the general symbol for a couple; as indeed is clear beforehand from the fact, that the couple is that which translates the force U from the point (u') to the point (u), *i. e.* along the line $(u-u')$.

It will be generally simpler to employ the symbol in the form

$$u.U,$$

u here denoting, symbolically, the line drawn from the point of application of the force $(-U)$ to that of the force (U). The *directrix* of this, *i. e.* the *axis of the couple*, is $D(u.U)$.

(76.) *To combine a given Set of Couples.*—Let us put (see art. 15)

$$u=x\alpha+y\beta+z\gamma$$

$$U=X\alpha+Y\beta+Z\gamma.$$

Then we find, by *lateral multiplication*,

$$u.U=(xY-yX)\alpha.\beta+(yZ-zY)\beta.\gamma+(zX-xZ)\gamma.\alpha.$$

Hence, if we suppose the given couples to be $u.U$, $u'.U'$, $u''.U''$, &c., and if we put, for brevity,

$$\Sigma(xY-yX)=N,\ \Sigma(yZ-zY)=L,\ \Sigma(zX-xZ)=M,$$

the combined effect of the couples will be

$$\Sigma(u.U)=L\beta.\gamma+M\gamma.\alpha+N\alpha.\beta$$
$$=D^{-1}(L\alpha+M\beta+N\gamma) \text{ (art. 48).}$$

Hence, if we assume G to denote the magnitude and γ' the direction of $L\alpha+M\beta+N\gamma$, which gives (art. 17)

$$G^2=L^2+M^2+N^2 \ldots\ldots (1.), \qquad \gamma'=\frac{L}{G}\alpha+\frac{M}{G}\beta+\frac{N}{G}\gamma \ldots\ldots (2.);$$

we find

$$\Sigma u.U=D^{-1}(G\gamma')$$
$$=G\alpha'.\beta' \text{ (arts. 48 and 16).}$$

Here $\alpha'.\beta'$ denotes a unit-couple in a plane at right angles to γ'; and thus the resultant is a couple G in this plane; G and γ' being given by (1.) and (2.).

(77.) *To combine a given Set of Forces acting at given Points of a Rigid Body.*—Let the forces be U, U', U,'' &c., and their points of application u, u', u'', &c.; then, by art. 74, their combined effect will be

$$\Sigma(U+u.U).$$

Hence, if we put

$$\Sigma X=X_l, \quad \Sigma Y=Y_l, \quad \Sigma Z=Z_l,$$

and employ the notation of the preceding article, the combined effect becomes

$$X_l\alpha+Y_l\beta+Z_l\gamma+G\alpha'.\beta',$$

or

$$R_l\alpha_l+G\alpha'.\beta',$$

where

$$R_l^2=X_l^2+Y_l^2+Z_l^2, \ldots\ldots\ldots\ldots (3.)$$

and

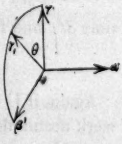
$$\gamma_l=\frac{X_l}{R_l}\alpha+\frac{Y_l}{R_l}\beta+\frac{Z_l}{R_l}\gamma. \ldots\ldots\ldots\ldots (4.)$$

Thus it appears that the set of given forces are combined into a single force $R_l\alpha_l$ (given by (3.) and (4.)), and a single couple $G\alpha'.\beta'$ (given by (1.) and (2.) previous article).

From the result just obtained the various well-known conditions and equations, relating to the effect of a set of forces on a rigid body, immediately follow. To exemplify the method I shall apply the formula $R_l\alpha_l+G\alpha'.\beta'$ to the following question.

(78.) *To combine the set of forces into Two Forces.*—Let us so choose α' and β' (which are arbitrary, except so far as they are perpendicular to γ'), that β' shall be in the same plane as γ_l and γ'; and let θ be the angle which γ_l makes with γ'. Then, by article 17,

Fig. 34.



$$\gamma_l=\gamma' \cos\theta+\beta' \sin\theta;$$
$$\therefore R_l\gamma_l+G\alpha'.\beta'=(R_l \cos\theta)\gamma'+(R_l \sin\theta)\beta'+G\alpha'.\beta'$$
$$=(R_l \cos\theta)\gamma'+(R_l \sin\theta)\beta'+\left(\frac{G}{R_l \sin\theta}\alpha'\right)\cdot(R_l \sin\theta)\beta'.$$

Now by article 74 this is the expression for the effect of two forces, viz.—

$R_l \cos\theta$ in the direction γ',

and $R_l \sin\theta$ in the direction β',

the former acting at the origin, and the latter at a point whose distance from the origin is

$$\frac{G}{R_i \sin\theta} \text{ drawn in the direction } \alpha'.$$

R_i, G, γ' and γ_i are given by the equations (1), (2), (3), (4) above; α' is determined because it is at right angles to the plane $(\gamma'\gamma_i)$, and β', because it is in the same plane, and at right angles to γ'. As regards θ, we have, taking the *longitudinal product* of (2.) and (4.),

$$\frac{LX_i+MY_i+NZ_i}{LR_i}=\gamma'\times\gamma_i=\cos\theta.$$

(79.) *Centre of Parallel Forces.*—Let the magnitudes of the parallel forces be R, R', R'', &c., γ_i their common direction, and u, u', u'', &c. their points of application. Then their combined effect is

$$\Sigma(R\gamma_i)+\Sigma(u.R\gamma_i)=(\Sigma R)\gamma_i+\frac{\Sigma Ru}{\Sigma R}\cdot(\Sigma R)\gamma_i.$$

Now, by art. 74, this is the symbol for a force ΣR, acting in the direction γ_i, and at a point whose distance from the origin is

$$\frac{\Sigma Ru}{\Sigma R},$$

or

$$\frac{\Sigma Rx}{\Sigma R}\alpha+\frac{\Sigma Ry}{\Sigma R}\beta+\frac{\Sigma Rz}{\Sigma R}\gamma,$$

which expresses the common formulæ for the centre of parallel forces.

III. Application of the Symbolic Forms to Dynamics.

(80.) *Effective Force, Vis-Viva, Work.*—If u be the distance of a moving particle m from the origin at any time t, it is clear that du represents, in magnitude and direction, the space described in the time dt; and thus the complete symbol of the velocity becomes

$$\frac{du}{dt}.$$

Also $d\left(\frac{du}{dt}\right)$ represents, in magnitude and direction, the alteration of velocity in the time dt; and thus the complete symbol of the effective force is

$$m\frac{d^2u}{dt^2}.$$

Again, if U denote any force acting on m, it is easy to see that the symbol of the work accumulated, while m is moving from the point u to the point u', is

$$\int_u^{u'} U\times du.$$

Lastly, if for U here we put the effective force, the *effective work* will be

$$\int_u^{u'} m\frac{d^2u}{dt^2}\times du$$

or* $$\frac{m}{2}\left(\frac{du'}{dt}\times\frac{du'}{dt}-\frac{du}{dt}\times\frac{du}{dt}\right).$$

Here $\frac{du}{dt}\times\frac{du}{dt}$ denotes the square of the velocity (art. 49), and thus the expression denotes the ordinary *vis-viva*.

(81.) *Description of Areas.*—It is clear that $\frac{1}{2}u.du$ is the area described in the time dt by u; for it is the half parallelogram formed on u and du. But it is to be noticed that $\frac{1}{2}u.du$ represents this area, not only in magnitude, but also *in position*.

If we put $\frac{1}{2}u.du=\mathrm{A}dt$, we find

$$\frac{d\mathrm{A}}{dt}=\frac{1}{2}\left(u.\frac{d^2u}{dt^2}+\frac{du}{dt}.\frac{du}{dt}\right), \text{ and } \frac{du}{dt}.\frac{du}{dt}=0 \text{ (art. 32)};$$

whence $$\frac{d\mathrm{A}}{dt}=\frac{1}{2}u.\frac{d^2u}{dt^2}. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

A, here, is a symbol which represents two important things:—1st, *in magnitude*, it is the ordinary *rate of description of area* $\left(r^2\frac{d\theta}{dt}\right)$; 2ndly, its plane is the plane containing the radius vector and the direction of motion, *i.e.* the *plane of the orbit* of m.

DA is the symbol of a line perpendicular to the plane of the orbit, and equal in magnitude to the rate of description of area.

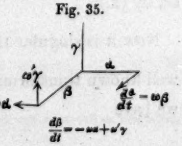
If we put $u=x\alpha+y\beta+z\gamma$, and perform the operation indicated in equation (1.), we find, $$2\frac{d(\mathrm{DA})}{dt}=\left(x\frac{d^2y}{dt^2}-y\frac{d^2x}{dt^2}\right)\gamma+\left(y\frac{d^2z}{dt^2}-z\frac{d^2y}{dt^2}\right)\alpha+\left(z\frac{d^2x}{dt^2}-x\frac{d^2z}{dt^2}\right)\beta.$$

(82.) *Expression for Effective Force with reference to radius vector, angular velocity, and plane of orbit.*—Let r denote the magnitude and α the "*direction*" of u; let β denote a direction *in* the plane of the orbit of m (and, of course, at right angles to α); then γ will be a direction always perpendicular to the plane of the orbit: lastly, let ω denote the angular velocity of u, and ω' the angular velocity of the plane of the orbit about u, ω and ω' being numerical quantities. On referring to art. 19, the following relations are manifest,

$$u=r\alpha \qquad \frac{d\alpha}{dt}=\omega\beta \qquad \frac{d\beta}{dt}=-\omega\alpha+\omega'\gamma;$$

for $\frac{d\alpha}{dt}$ and $\frac{d\beta}{dt}$ denote, in magnitude and direction, the velocities of the extremities of the directed units α and β, and the figure will show what their values are in terms of ω and ω'.

Fig. 35.



* $$d\left(\frac{du}{dt}\times\frac{du}{dt}\right)=\frac{d^2u}{dt}\times\frac{du}{dt}+\frac{du}{dt}\times\frac{d^2u}{dt}$$
$$=2\frac{d^2u}{dt^2}\times du \text{ (art. 33)}.$$

Hence we immediately find the required expression for the effective force $\frac{d^2u}{dt^2}$ by simple differentiation, as follows,

$$\frac{du}{dt}=\frac{dr}{dt}\alpha+r\frac{d\alpha}{dt}=\frac{dr}{dt}\alpha+r\omega\beta$$

$$\frac{d^2u}{dt^2}=\frac{d^2r}{dt^2}\alpha+\frac{dr}{dt}\frac{d\alpha}{dt}+\frac{d(r\omega)}{dt}\beta+r\omega\frac{d\beta}{dt},$$

$$\text{or effective force}\quad=\left(\frac{d^2u}{dt^2}-r\omega^2\right)\alpha+\left(\frac{dr}{dt}\omega+\frac{d(r\omega)}{dt}\right)\beta+r\omega\omega'\gamma.$$

Hence it appears that the effective force is equivalent to the well-known expressions along and perpendicular to the radius vector, together with a third part, $r\omega\omega'$ per pendicular to the plane of the orbit.

(83.) *The use of the forms* u.v *and* u×v *exemplified in the case of motion about a centre of force varying as* r^{-2}.—This case appears to me to afford so good an illustration of the use of these forms, that I shall give it here briefly. Assuming α, β, γ, ω, r as in the preceding article, it is clear that the symbol of the central force is $-\frac{\mu}{r^2}\alpha$, and therefore we have

$$\frac{d^2u}{dt^2}=-\frac{\mu}{r^3}\alpha, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

whence $u.\frac{d^2u}{dt^2}=0$, since $u=r\alpha$, and $\alpha.\alpha=0$,

therefore (by art. 81), $\frac{dA}{dt}=0$. $\ldots\ldots\ldots\ldots\ldots\ldots$ (2.)

This indicates that there is no variation of A, and consequently (see art. 81) that the plane of the orbit, and the rate of description of area is constant.

If we put for A its value (art. 81) $\frac{1}{2}u.\frac{du}{dt}$ and for u and $\frac{du}{dt}$ their values $r\alpha$ and $\frac{d(r\alpha)}{dt}$, observing that $\frac{d\alpha}{dt}=\omega\beta$, we find

$$A=\frac{1}{2}r\alpha.\left(\frac{dr}{dt}\alpha+r\omega\beta\right)=\frac{1}{2}r^2\omega\alpha.\beta,$$

or, by (2.), $A=\frac{1}{2}h\alpha.\beta$ $(h=r^2\omega$, as usual).

Now it is singular that (1.) admits of *immediate integration*, instead of requiring the well-known transformations: for, in it, put for α its value, $-\frac{1}{\omega}\frac{d\beta}{dt}$ (see art. 82), and we have

$$\frac{d^2u}{dt^2}=\frac{\mu}{r^2\omega}\frac{d\beta}{dt}=\frac{\mu}{h}\frac{d\beta}{dt};$$

wherefore, integrating, we find

$$\frac{du}{dt}=\frac{\mu}{h}\beta+\text{ constant}.$$

Here put for $\frac{du}{dt}$ its value, and there results

$$\frac{dr}{dt}\alpha+r\omega\beta=\frac{\mu}{h}\beta+\text{ constant }(c\beta' \text{ suppose}^*).$$

Multiply this *longitudinally* by β, and, since $\beta\times\alpha=0$, $\beta\times\beta=1$, and $\beta\times\beta'=\alpha\times\alpha'=\cos\theta$, we find

$$r\omega=\frac{\mu}{h}+c\cos\theta;$$

or, since $r\omega=\frac{h}{r}$, this gives

$$\frac{1}{r}=\frac{\mu}{h^2}+\frac{c}{h}\cos\theta,$$

the well-known equation, θ being the angle which α (the direction of the radius vector) makes with α' (an arbitrary constant direction).

The manner in which the symbolic forms have effected this integration appears to me to be worthy of notice.

(84.) *General Expression for the Momentum of a Rigid Body moving in any manner.*—The *Momentum* of a particle (m), moving with a velocity $\left(\frac{du}{dt}\right)$, is

$$m\frac{du}{dt}.$$

And this symbol represents the Momentum in direction as well as magnitude. Now momentum is really *a force*, estimated, however, somewhat differently from ordinary pressure, on the principle that the true *dynamical effect* of a force is proportional to absolute intensity and the time of its action conjointly†. Hence, regarding the momentum of m as a force, its complete symbol will be (by art. 74),

$$\frac{mdu}{dt}+u.\left(\frac{mdu}{dt}\right).$$

Thus the symbol of the *total momentum* of the rigid body will be

$$\Sigma m\left\{\frac{du}{dt}+u.\frac{du}{dt}\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

(85.) If $\bar{u}$ denote the distance of the *centre of gravity* from the origin, and u' the distance of the point (u) from the centre of gravity, we have $u=\bar{u}+u'$, and thus, since $\Sigma mu'=0$, (1.) becomes (putting M for Σm)

$$M\frac{d\bar{u}}{dt}+\bar{u}.\left(M\frac{d\bar{u}}{dt}\right)+\Sigma m\left(u'.\frac{du'}{dt}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

* Of course the constant is the symbol for some constant line, as regards *direction* as well as magnitude, and therefore I put it in the form $c\beta'$, c being a number and β' a "*direction*."

† If the *pressure* P produces a velocity v in the time t, it produces the velocity $\frac{v}{t}$ per second, and therefore $P=m\frac{v}{t}$; or $mv=Pt$. Pt then is the momentum, and this is proportional to P and t conjointly.

Now here, by (1.), $M\frac{d\bar{u}}{dt}+\bar{u}.\left(M\frac{d\bar{u}}{dt}\right)$ is the expression for the momentum of M concentrated into the centre of gravity, and $\Sigma m\left(u'.\frac{du'}{dt}\right)$ is the momentum of the body as regards its motion *relatively* to the centre of gravity (as is evident from (1.), for, if the origin have no momentum, $\Sigma m\frac{du}{dt}=0$).

In Section V. I shall show, that

$$\Sigma m\left(u'.\frac{du'}{dt}\right)=D^{-1}(A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma),$$

that is, *a couple whose axis* is $A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma$. Here A, B, C are the moments of inertia about the three principal axes, of which α, β, γ are supposed to be the directions; and ω_1, ω_2, ω_3 are the well-known component angular velocities.

(86.) *General Expression for the Energy of a Rigid Body moving in any manner.*—I venture to suggest the word "*energy*" as a proper designation for the *total effective force* by which the motion of a rigid body is produced, inasmuch as ἐνέργεια means force *actually exerted and effective*. The symbol, then, of *the energy* of a rigid body will be

$$\Sigma m\left(\frac{d^2u}{dt^2}+u.\frac{d^2u}{dt^2}\right).$$

Now, observing that $\frac{du}{dt}.\frac{du}{dt}=0$, we have

$$u.\frac{d^2u}{dt^2}=d\left(u.\frac{du}{dt}\right).$$

Hence the expression for the energy is

$$\frac{d}{dt}\left\{\Sigma m\left(\frac{du}{dt}+u.\frac{du}{dt}\right)\right\}.$$

Comparing this with the expression for the *momentum* in art. 84, we have

$$energy=\frac{d\,(momentum)}{dt} \quad \ldots\ldots\ldots\ldots \quad (1.)$$

(87.) This, though a very simple result, is really one of importance; thus if the centre of gravity be fixed, we find (by art. 85.)

$$energy=D^{-1}\frac{d}{dt}(A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma).$$

Now it will be shown in Sect. V. that

$$\frac{d\alpha}{dt}=D\omega.\alpha,\quad \frac{d\beta}{dt}=D\omega.\beta,\quad \frac{d\gamma}{dt}=D\omega.\gamma,$$

where $\omega=\omega_1\alpha+\omega_2\beta+\omega_3\gamma$.

Hence, performing the differentiation, and observing that

$$D^{-1}\alpha=\beta.\gamma,\ D^{-1}\beta=\gamma.\alpha,\ D^{-1}\gamma=\alpha.\beta,\text{ and } D^{-1}D=1,$$

we find
$$\text{energy} = A\frac{d\omega_1}{dt}\beta.\gamma + B\frac{d\omega_2}{dt}\gamma.\alpha + C\frac{d\omega_3}{dt}\alpha.\beta + \omega.(A\omega_1\alpha + B\omega_2\beta + C\omega_3\gamma).$$

Lastly, if we perform the *lateral multiplication* denoted by ω. here, we find
$$\text{energy} = \left\{A\frac{d\omega_1}{dt} + (C-B)\omega_2\omega_3\right\}\beta.\gamma + \left\{B\frac{d\omega_2}{dt} + (A-C)\omega_3\omega_1\right\}\gamma.\alpha + \left\{C\frac{d\omega_3}{dt} + (B-A)\omega_1\omega_2\right\}\alpha.\beta.$$

I need not point out the meaning of this formula in relation to EULER's equations for a rigid body.

(88.) *General Expression for the "Vis-Mortua" or "Dead Pull" on a Rigid Body.*—I may use the almost obsolete word "*Vis-Mortua*" (which has been so well translated by the familiar expression "*Dead Pull*") in the sense in which it was originally employed to denote simple pressure or *impressed force*. I shall therefore designate the total mechanical effect of the impressed forces acting on a rigid body as the *Vis-Mortua* or *Dead Pull* on that body. If U denote the force acting at the point (u), we have, therefore, by art. 74,
$$\textit{Vis-Mortua} = \Sigma(U + u.U):$$
this of course is the same expression as that in art. 77.

If we put, as before, $u = \bar{u} + u'$, this expression becomes
$$\Sigma U + \bar{u}.\Sigma U + \Sigma u'.U.$$
Here $\Sigma U + \bar{u}.\Sigma U$ is the symbol of the force ΣU acting at the point $(\bar{u})$, and $\Sigma u'.U$ is (art. 75) the symbol of a couple.

(89.) As an example, the result of which I shall require in Sect. V., I shall calculate the Vis-Mortua of a rigid body acted on by the attraction of a distant particle m', taking the centre of gravity of the rigid body as origin, and α, β, γ as the directions of the three principal axes; assuming also u' to denote the distance of m', r' the magnitude of u', and r that of u.

Here U is a force acting along the line joining the two points (u) and (u'), and inversely proportional to the square of the magnitude of that line. The line alluded to is $u' - u$, its magnitude is $\sqrt{(u'-u)\times(u'-u)}$, and its direction therefore is
$$\frac{u'-u}{\sqrt{(u'-u)\times(u'-u)}}.$$

Hence
$$U = mm'\frac{(u'-u)}{\{(u'-u)\times(u'-u)\}^{\frac{3}{2}}}.$$

Also
$$\{(u'-u)\times(u'-u)\}^{-\frac{3}{2}} = (u'\times u' - 2u'\times u + u\times u)^{-\frac{3}{2}} = (r'^2 - 2u'\times u + r^2)^{-\frac{3}{2}} = \frac{1}{r'^3}\left(1 + \frac{3u'\times u}{r'^2}\right) \text{ nearly.}$$

Hence, observing that $u.u=0$, and $\Sigma mu=0$, we have

$$\Sigma u.\mathrm{U}=\frac{3m'}{r'^5}\Sigma m(u'\times u)(u.u')$$

$$=-\frac{3m'}{r'^5}u'.\{\Sigma m(u'\times u)u\}.$$

Now $\Sigma m(u'\times u)u=\Sigma m(xx'+yy'+zz')(x\alpha+y\beta+z\gamma)$

$=(\Sigma mx^2)x'\alpha+(\Sigma my^2)y'\beta+(\Sigma mz^2)z'\gamma$, by properties of principal axes,

$=(\Sigma mr^2-\mathrm{A})x'\alpha+(\Sigma mr^2-\mathrm{B})y'\beta+(\Sigma mr^2-\mathrm{C})z'\gamma$

$=(\Sigma mr^2)u'-(\mathrm{A}x'\alpha+\mathrm{B}y'\beta+\mathrm{C}z'\gamma)$.

Wherefore, observing that $u'.u'=0$, we find

$$\Sigma u.\mathrm{U}=\frac{3m'}{r'^5}u'.(\mathrm{A}x'\alpha+\mathrm{B}y'\beta+\mathrm{C}z'\gamma).$$

Also $\Sigma\mathrm{U}=\Sigma mm'\frac{u'-u}{r'^3}$ (neglecting $\frac{3u'\times u}{r'^3}$ for obvious reasons)

$$=\mathrm{M}m'\frac{u'}{r'^3} \qquad \begin{cases}\mathrm{M}=\Sigma m\\ \Sigma mu=0.\end{cases}$$

Hence we have

$$\textit{Vis-Mortua}=\mathrm{M}m'\frac{u'}{r'^3}+\frac{3m'}{r'^5}u'.(\mathrm{A}x'\alpha+\mathrm{B}y'\beta+\mathrm{C}z'\gamma),$$

which expresses a force $\mathrm{M}m'\frac{u'}{r'^3}$ acting at the origin, and a couple,

$$\frac{3m'}{r'^5}u'.(\mathrm{A}x'\alpha+\mathrm{B}y'\beta+\mathrm{C}z'\gamma).$$

If we perform the *lateral multiplication* indicated by $(u'.)$, this couple becomes

$$\frac{3m'}{r'^5}\Big\{(\mathrm{C}-\mathrm{B})y'z'\beta.\gamma+(\mathrm{A}-\mathrm{C})z'x'\gamma.\alpha+(\mathrm{B}-\mathrm{A})x'y'\alpha.\beta\Big\},$$

which gives the three well-known couples in the theory of Precession and Nutation.

IV. Application of the Symbolic Forms to determine the Correction for the Earth's Rotation in Problems relating to Motion on or near the Earth's Surface.

(90.) The best way of defining that which is commonly called the *Centrifugal Force* appears to be the following, viz. that it is an imaginary force which may be introduced as a correction for the error of not taking into account the rotation of the radius vector r. Suppose P to denote the accelerating force acting along r, and let us for a moment forget that r has an angular velocity $\left(\frac{d\theta}{dt}\right)$, then we put

$$\frac{d^2r}{dt^2}=\mathrm{P};$$

but this is erroneous, and we must correct it for the rotation by adding to P the term $r\left(\frac{d\theta}{dt}\right)^2$, as is well known. Hence we may regard the centrifugal force as a *correction for neglected rotation.*

But it only corrects the error so far as the motion along r is concerned; another correction (supposing still the rotation is neglected or forgotten) is necessary to be applied at right angles to r, namely, the imaginary force $-\frac{1}{r}\frac{d}{dt}\left(\frac{r^2d\vartheta}{dt}\right)$. Thus the true and complete correcting force is the resultant of the two forces

$$r^2\frac{d\vartheta}{dt}, \text{ and } -\frac{1}{r}\frac{d}{dt}\left(\frac{r^2d\vartheta}{dt}\right).$$

It appears to me that the idea here suggested might be applied with great advantage to cases of motion on or near the earth's surface. The beautiful pendulum experiment which made so much noise last year, and the various investigations respecting it, give great interest to such cases of motion. I propose therefore to investigate here, by the aid of the Symbolic Forms, the proper symbol of the imaginary force which corrects completely for the earth's rotation supposed to be neglected. By the aid of this symbol, it will be found that the greatest possible simplicity is introduced into investigations such as those relating to the pendulum experiment. It will enable the investigator to forget altogether the earth's rotation in framing his equations of motion, and at the same time to *correct his error by the introduction of a simple term.*

(91.) Let ω denote a line pointing in the direction of the earth's polar axis (north suppose), and representing, by its length, the earth's angular velocity. In other words, let ω be the *directrix of the earth's rotation,* then (as will be shown in Sect. V.) it is easy to see, that, if u denote (symbolically) the distance of any point from the earth's centre, the velocity communicated to it by the earth's rotation (if it be fixed to the earth) is represented by the symbol*

$$\mathrm{D}\omega.u.$$

Now let d denote differentiation (of u) on the erroneous supposition that the earth is fixed, and δ the true and complete differentiation; then the true velocity of the point u is $\frac{\delta u}{dt}$, and this must be the resultant of the erroneous velocity $\left(\frac{du}{dt}\right)$ and the velocity $(\mathrm{D}\omega.u)$ due to the rotation. Hence we have

$$\frac{\delta u}{dt}=\frac{du}{dt}+\mathrm{D}\omega.u.$$

The effective accelerating force will be obtained by the true and complete differentiation of the correct velocity, that is,

$$\frac{\delta}{dt}\left(\frac{du}{dt}+\mathrm{D}\omega.u\right),$$

or

$$\frac{d}{dt}\left(\frac{\delta u}{dt}\right)+\mathrm{D}\omega.\frac{\delta u}{dt} \quad \text{(observing that } \omega \text{ is a constant),}$$

* For $\mathrm{D}\omega.u$ denotes a line at right angles to both ω and u, and its magnitude is $\omega r\sin\theta$; where ω is the magnitude of ω, r that of u, and θ the angle which u makes with ω. Therefore $\mathrm{D}\omega.u$ is manifestly the velocity caused in the point u by the rotation ω.

which, putting for $\frac{\delta u}{dt}$ the above value, becomes

$$\frac{d^2u}{dt^2}+2D\omega.\frac{du}{dt}+D\omega.(D\omega.u).$$

Hence if U denote (symbolically) the resultant of the accelerating forces, whatever they may be, which act on the point (u), we find

$$\frac{d^2u}{dt^2}+2D\omega.\frac{du}{dt}+(D\omega.)^2u=U. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

I may observe, in passing, that, since $d+(D\omega.)$ represents the *complete* differentiation of u, we might have written down the equation of motion *immediately*, in the form

$$\frac{\{d+(D\omega.)\}^2}{dt^2}u=U,$$

which, expanded, is identical with (1.).

Now, if we had forgotten the earth's rotation, we should have put, instead of (1.),

$$\frac{d^2u}{dt^2}=U.$$

Hence it appears that, if we neglect the rotation in forming the equation of motion, we may *correct the error*, by supposing that there is the imaginary force

$$-\left\{2D\omega.\frac{du}{dt}+(D\omega.)^2u\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

acting in addition to the real force represented by U; for on this supposition we find

$$\frac{d^2u}{dt^2}=U-2D\omega.\frac{du}{dt}-(D\omega.)^2u, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

which is equivalent to (1.).

As regards terrestrial problems, however, the expression (2.) admits of an important simplification; for the accelerating force of gravity (g), which of course is included in U, is supposed to be the resultant of the earth's attraction, and the *common centrifugal force*. Now this common centrifugal force is that which is conceived to be in action upon a particle rigidly connected with the revolving earth; in other words, it is what (2.) becomes when $\frac{du}{dt}=0$. Wherefore the expression for the *common centrifugal force* is

$$-(D\omega.)^2u. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

As this therefore is included among the forces which U represents, it ought to be omitted in the equation (3.). Thus we find that

$$-2D\omega.\frac{du}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

is the force which must be supposed to act on the point (u) *as a correction for the neglected rotation.*

We may, therefore, in all problems of motion relative to the earth, forget altoge-

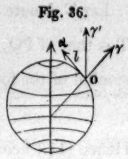
ther the earth's rotation, provided we introduce the force (5.), in addition to the accelerating forces, whatever they may be, which are really in action upon the point (u); it being understood that the common centrifugal force is allowed for in g.

(92.) In (5.) $\frac{du}{dt}$ is the apparent velocity of the point u (apparent, that is, to an observer unconscious of the earth's motion); and if n denote the earth's angular velocity, and γ' the direction of the polar axis, $\omega = n\gamma'$. Thus (5.) becomes

$$-2n\mathrm{D}\gamma' . \frac{du}{dt} . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

Now this represents a force at right angles to γ' and $\frac{du}{dt}$, *i. e.* to the *polar plane* in which the apparent velocity is taking place at the instant t. Also the magnitude of this force is $2n$ times the apparent velocity $\left(\frac{du}{dt}\right)$ multiplied by the sine of the angle it $\left(\frac{du}{dt}\right)$ makes with the polar axis (γ').

Fig. 36.



(93.) This force may be expressed with reference to horizontal and vertical coordinates at any place, as follows:

Let O be the place, γ the vertical at O, and then the plane ($\alpha\beta$) will be horizontal: also let α be chosen so as to lie in the meridian plane; and let l denote the latitude of O (*i. e.* the angle γ' makes with α). Then

$$\gamma' = \alpha \cos l + \gamma \sin l . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

Also if we take u to denote the distance of the moving point from O at any time t, and therefore put

$$u = x\alpha + y\beta + z\gamma , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

$\frac{du}{dt}$ will be the same as the $\frac{du}{dt}$* in (6.), for all that we have to express by $\frac{du}{dt}$ is the apparent velocity of the point u.

Hence, differentiating (8.), and performing the operations indicated by $2n\mathrm{D}\gamma'$., (6.) becomes

$$-2n\mathrm{D}(\alpha \cos l + \gamma \sin l) . \left(\frac{dx}{dt}\alpha + \frac{dy}{dt}\beta + \frac{dz}{dt}\gamma\right),$$

or

$$2n\left\{\left(\frac{dy}{dt}\sin l\right)\alpha + \left(-\frac{dx}{dt}\sin l + \frac{dz}{dt}\cos l\right)\beta - \left(\frac{dy}{dt}\cos l\right)\gamma\right\} . \quad . \quad . \quad . \quad (9.)$$

Hence the ordinary equations of motion will be

$$\left.\begin{aligned} \frac{d^2x}{dt^2} &= \mathrm{X} + 2n\frac{dy}{dt}\sin l \\ \frac{d^2y}{dt^2} &= \mathrm{Y} - 2n\frac{dx}{dt}\sin l + 2n\frac{dz}{dt}\cos l \\ \frac{d^2z}{dt^2} &= \mathrm{Z} - 2n\frac{dy}{dt}\cos l \end{aligned}\right\} . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

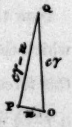
* Originally u was measured from the earth's centre.

where X, Y, Z represent the accelerating forces, whatever they may be, that are in action on the point u.

(94.) The formation of these equations, here given, affords a good example of the use of the symbolic form $u.v$: but, to illustrate the method more clearly, it will be worth while to employ, in some particular problem, the general equation,

$$\frac{d^2u}{dt^2}=\mathrm{U}-2n\mathrm{D}\gamma'.\frac{du}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

which includes the three equations (10.), U denoting $\mathrm{X}\alpha+\mathrm{Y}\beta+\mathrm{Z}\gamma$. The problem I shall choose will be that of the *Pendulum Experiment*. Fig. 37.



Let QP represent the string at any time t, QO its vertical position, c its length; then

$$\mathrm{OQ}=c\gamma, \quad \mathrm{OP}=u, \quad \mathrm{PQ}=c\gamma-u,$$

als

$$\textit{direction of PQ}=\frac{c\gamma-u}{c}.$$

Let T denote (in magnitude) the tension of PQ; and then, since the direction of T is that of PQ, the symbol of the tension is $\mathrm{T}\frac{c\gamma-u}{c}$; to which if we add $-g\gamma$, the symbol of the force of gravity, we find

$$\mathrm{U}=\mathrm{T}\frac{c\gamma-u}{c}-g\gamma=(\mathrm{T}-g)\gamma-\frac{\mathrm{T}}{c}u.$$

Hence (11.) becomes

$$\frac{d^2u}{dt^2}=(\mathrm{T}-g)\gamma-\frac{\mathrm{T}}{c}u-2n\mathrm{D}\gamma'.\frac{du}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

Now, for greater simplicity, I shall suppose that u represents a small excursion, and c a long string. On this supposition we may regard u as always horizontal. Also, if we put for γ' its value (7.), the equation (12.) becomes

$$\left\{\frac{d^2u}{dt^2}+2n\sin l\mathrm{D}\gamma.\frac{du}{dt}-\frac{\mathrm{T}}{c}u\right\}+\left\{2n\cos l\mathrm{D}\alpha.\frac{du}{dt}-(\mathrm{T}-g)\gamma\right\}=0.$$

Hence, since $\frac{du}{dt}$ is horizontal, $\mathrm{D}\alpha.\frac{du}{dt}$ is vertical, and $\mathrm{D}\gamma.\frac{du}{dt}$ is horizontal. The equation (12.), therefore, is separated into two parts, horizontal and vertical, which, being equated to zero, we obtain

$$\frac{d^2u}{dt^2}+2n\sin l\mathrm{D}\gamma.\frac{du}{dt}-\frac{\mathrm{T}}{c}u=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

$$(\mathrm{T}-g)\gamma=2n\cos l\mathrm{D}\alpha.\frac{du}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

We have to substitute for T in (13.) its value derived from (14.), which on account of the smallness of n, and the fact that T is multiplied by $\frac{u}{c}$ in (13.), gives $\mathrm{T}=g$ for a first approximation. Hence (13.) becomes

$$\frac{d^2u}{dt^2}+2n\sin l\mathrm{D}\gamma.\frac{du}{dt}-\frac{g}{c}u=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

Now, here, the operation $(D\gamma.)$ is performed only on lines at right angles to γ; we may therefore put $\sqrt{-1}$ for $(D\gamma.)$ (see art. 55); and thus (15.) becomes

$$\frac{d^2u}{dt^2}+2n\sin l\sqrt{-1}\frac{du}{dt}-\frac{g}{c}u=0. \quad \ldots\ldots\ldots \quad (16.)$$

The roots of the equation

$$\left(\frac{d}{dt}\right)^2+2n\sin l\sqrt{-1}\left(\frac{d}{dt}\right)-\frac{g}{c}=0$$

are $$-n\sin l\sqrt{-1}\pm\sqrt{-n^2\sin^2 l-\frac{g}{c}},$$

or $$(-n\sin l\pm m)\sqrt{-1},$$

if, for brevity, we put $n^2\sin^2 l+\frac{g}{c}=m^2$. Hence the solution of (16.) is

$$u=\varepsilon^{-nt\sin l\sqrt{-1}}(A\varepsilon^{mt\sqrt{-1}}+B\varepsilon^{-mt\sqrt{-1}}). \quad \ldots\ldots\ldots \quad (17.)$$

The constants A and B here denote two arbitrary lines in the horizontal plane.

If the earth were fixed, the form of the solution would have been

$$u=A\varepsilon^{mt\sqrt{-1}}+B\varepsilon^{-mt\sqrt{-1}} \qquad \left\{\text{for then } m^2=\frac{g}{c}\right\}.$$

Now $\varepsilon^{-nt\sin l\sqrt{-1}}$* indicates a uniform *backward* rotation of $A\varepsilon^{mt\sqrt{-1}}+B\varepsilon^{-mt\sqrt{-1}}$ with an angular velocity $n\sin l$. Thus it appears that the apparent curve of motion of the pendulum will be the same form as if the earth were fixed, only there will be a slow angular regression of the whole about the vertical as axis at the rate $n\sin l$ per second.

I may observe, in passing, that the simplest interpretation of (17.) is this; that the motion of the point (u) results from the superposition of two motions,

$$A\varepsilon^{(m-n\sin l)t\sqrt{-1}} \text{ and } B\varepsilon^{-(m+n\sin l)t\sqrt{-1}};$$

and these are two uniform circular* motions, the former that of the line A forward with an angular velocity $(m-n\sin l)$; the latter that of B backward with an angular velocity $m-n\sin l$.

(95.) As my object is simply to exemplify the application of my notation, I shall not proceed to a second approximation; which however is very easily effected by substituting for T in (13.) its complete value given by (14.), after having put for u in (14.) the value (17.) just obtained. The result is important, especially as regards motion near the equator.

* $A\varepsilon^{\theta\sqrt{-1}}$ denotes A turned out of its position (round γ) through an angle θ, and therefore $u=A\varepsilon_{nt\sqrt{-1}}$ is an equation indicating that the motion of the point (u) results from rotation round the origin at the distance A, the angle nt being described in the time t.

V. Application of the Symbolic Forms to determine the Motion of a Rigid Body about its Centre of Gravity.

(96.) The symbolic forms $u.v$ and $u \times v$ are singularly useful, as it appears to me, in all cases of the Motion of a Rigid Body in space, especially as regards Rotation. Considerable simplification is also gained by employing $d\alpha$, $d\beta$, $d\gamma$ to denote the angular motions of the three axes α, β, γ. I shall now proceed to consider this case.

I shall take α, β, γ to denote three rectangular directions fixed in the Rigid Body, and x, y, z the coordinates of any particle (m) of the body. On this supposition x, y, z are constants as regards t, while α, β, γ are variables. The origin is the fixed point (the centre of gravity, namely,) about which the body moves. u denotes the distance of m from the origin, and therefore

$$u = x\alpha + y\beta + z\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

(97.) Now the rigidity of the body requires that the velocity $\left(\frac{du}{dt}\right)$ of m shall be at right angles to u always; this may be expressed (see art. 44) by putting

$$\frac{du}{dt} = \mathrm{D}\omega.u, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

where ω denotes some unknown line. It may be shown, as follows, that ω is a function of t *only*, or, in other words, that ω is the same for all points of the body, *i. e.* for all values of u.

Let (u') be *any point in space*, and let us assume, as we may, that this point moves always with a velocity $\mathrm{D}\omega.u'$; then

$$\frac{du'}{dt} = \mathrm{D}\omega.u'; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

and hence, by (2),

$$\frac{d(u'-u)}{dt} = \mathrm{D}\omega.(u'-u). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

Now $u'-u$ is the line joining the two points (u) and (u'), and the square of its length is

$$(u'-u) \times (u'-u),$$

and

$$\frac{d}{dt}\{(u'-u) \times (u'-u)\} = 2\,\frac{d(u'-u)}{dt} \times (u'-u) = 0;$$

for (4.) shows that $\frac{d(u'-u)}{dt}$ and $u'-u$ are at right angles. Consequently the length of the line $u'-u$ is invariable.

In precisely the same way (3.) shows (what indeed is otherwise obvious) that the line u' is of invariable length.

Hence the point (u') is rigidly connected with the origin and with the point (u); and consequently (u') *is a point of the rigid body*. Therefore, comparing (2.) and (3.), it appears that ω does not vary when we pass from one point to another of the rigid body.

This result is of great importance, and furnishes, in the simplest possible manner, every formula necessary for determining the motion of the rigid body.

(98.) *The symbol ω represents, in direction,* the *instantaneous axis of rotation.* For (2.) shows that, when u coincides in direction with ω, $\frac{du}{dt}=0$; consequently all the points of the body which lie in the direction of ω are quiescent at the instant t.

(99.) *The magnitude of ω is the instantaneous angular velocity.* For, let α' denote any unit line fixed in the body at right angles to ω; then (see art. 19) $\frac{d\alpha'}{dt}$ is the angular velocity, at least in magnitude. But, by (2.), $\frac{d\alpha'}{dt}=\mathrm{D}\omega.\alpha'$; and, since ω is at right angles to α', $\mathrm{D}\omega.\alpha'$ has the same magnitude as ω (see art. 40.). Wherefore ω has the same magnitude as $\frac{d\alpha'}{dt}$, and therefore represents the angular velocity in magnitude.

(100.) Hence the result above obtained, namely,

$$\frac{du}{dt}=\mathrm{D}\omega.u, \quad \ldots \ldots \ldots \ldots \ldots \quad (4.)$$

may be thus enunciated:—the rigid body is, at the time t, moving about a certain instantaneous axis, with a certain angular velocity; and if we assume ω to denote that axis, *in direction,* and the angular velocity *in magnitude,* then the velocity $\left(\frac{du}{dt}\right)$ of any point (u) of the rigid body is obtained by performing the operation $(\mathrm{D}\omega.)$ upon u.

(101.) If we put

$$\omega=\omega_1\alpha+\omega_2\beta+\omega_3\gamma, \quad \ldots \ldots \ldots \ldots \quad (5.)$$

where ω_1, ω_2, ω_3 denote numerically the projections of the line ω on the three coordinate directions α, β, γ, we find by (4.),

$$\frac{du}{dt}=\omega_1\mathrm{D}\alpha.u+\omega_2\mathrm{D}\beta.u+\omega_3\mathrm{D}\gamma.u. \quad \ldots \ldots \ldots \quad (6.)$$

Hence it appears that $\frac{du}{dt}$ results from the superposition of three angular velocities ω_1, ω_2, ω_3 about the axes α, β, γ respectively: for, by (4.), $\mathrm{D}(\omega_1\alpha).u$ (or $\omega_1\mathrm{D}\alpha.u$) denotes a velocity of the point (u) resulting from an angular velocity ω_1 about the axis α, $\omega_2\mathrm{D}\beta.u$ that resulting from ω_2 about β, and $\omega_3\mathrm{D}\gamma.u$ that resulting from ω_3 about γ: and (6.) shows that the actual velocity of u is the resultant of these three velocities.

If we put for u its value $x\alpha+y\beta+z\gamma$, (6.) becomes immediately

$$\frac{du}{dt}=(\omega_1 y-\omega_2 x)\gamma+(\omega_3 z-\omega_1 y)\alpha+(\omega_2 x-\omega_3 z)\beta.$$

Whence it follows that the velocity of the point (xyz) is equivalent to the three component velocities $\omega_2 z-\omega_3 y$ parallel to x, $\omega_3 x-\omega_1 z$ parallel to y, and $\omega_1 y-\omega_2 x$ parallel to z.

(102.) The theory of the composition and resolution of angular velocities is com-

pletely expressed by (4.); for, if it be required to find the effect of the superposition of two angular velocities represented by ω and ω', we find by (4.) that the velocity produced by ω in any point (u) is $\mathrm{D}\omega.u$, and that produced by ω' is $\mathrm{D}\omega'.u$. The actual velocity of (u) will be the resultant of these, that is,

$$\mathrm{D}\omega.u+\mathrm{D}\omega'.u, \text{ or } \mathrm{D}(\omega+\omega').u.$$

Now by (4.) $\mathrm{D}(\omega+\omega').u$ is the effect of an angular velocity represented by $\omega+\omega'$. Hence it follows that the two angular velocities ω and ω' superposed produce the same effect as the angular velocity $\omega+\omega'$; and $\omega+\omega'$ is the third side of the triangle formed of the two lines ω and ω'.

(103.) The equation of motion of a rigid body acted on by any forces (about its centre of gravity) is easily obtained as follows.

Let U denote the accelerating force in action on m, *i. e.* at the point (u); then, by Sect. III., we have

$$\Sigma mu.\frac{d^2u}{dt^2}=\Sigma mu.\mathrm{U},$$

which is the same thing (see art. 86) as

$$\frac{d}{dt}\left(\Sigma mu.\frac{du}{dt}\right)=\Sigma mu.\mathrm{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

Now, putting for u its value (1.), and supposing that α, β, γ are the *principal axes* of the body, we find

$$\Sigma mu.\frac{du}{dt}=\left(\alpha.\frac{d\alpha}{dt}\right)\Sigma mx^2+\left(\beta.\frac{d\beta}{dt}\right)\Sigma my^2+\left(\mathrm{D}\gamma.\frac{d\gamma}{dt}\right)\Sigma mz^2.$$

But, by (4.) and (5.),

$$\frac{d\alpha}{dt}=\mathrm{D}\omega.\alpha=-\omega_3\gamma+\omega_2\beta;$$

$$\therefore \quad \mathrm{D}\alpha.\frac{d\alpha}{dt}=\omega_2\beta+\omega_3\gamma^*.$$

Similarly,
$$\mathrm{D}\beta.\frac{d\beta}{dt}=\omega_3\gamma+\omega_1\alpha,$$

and
$$\mathrm{D}\gamma.\frac{d\gamma}{dt}=\omega_1\alpha+\omega_2\beta;$$

whence, introducing D, we find

$$\Sigma m\mathrm{D}u.\frac{du}{dt}=(\omega_2\beta+\omega_3\gamma)\Sigma mx^2+(\omega_3\gamma+\omega_1\alpha)\Sigma my^2+(\omega_1\alpha+\omega_2\beta)\Sigma mz^2=\mathrm{A}\omega_1\alpha+\mathrm{B}\omega_2\beta+\mathrm{C}\omega_3\gamma,$$

where A, B and C denote respectively $\Sigma m(y^2+z^2)$, $\Sigma m(z^2+x^2)$, $\Sigma m(x^2+y^2)$.

Thus (7.) becomes

$$\frac{d}{dt}(\mathrm{A}\omega_1\alpha+\mathrm{B}\omega_2\beta+\mathrm{C}\omega_3\gamma)=\Sigma m\mathrm{D}u.\mathrm{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

* Or thus: $\mathrm{D}\alpha.\frac{d\alpha}{dt}=\mathrm{D}\alpha.(\mathrm{D}\omega.\alpha)=-(\mathrm{D}\alpha.)^2\omega=\omega-(\omega\times\alpha)\alpha=\omega_1\alpha+\omega_2\beta.$

This is the general equation of motion of a rigid body about a fixed point. It gives the three well-known equations immediately by equating the coefficients of α, β, γ*. But the equation (8.) as it stands is more available for the solution of problems, and furnishes results much more simply, than the three equations alluded to. Along with (8.) we must employ the equation

$$\frac{du}{dt} = \mathrm{D}\omega . u. \quad \ldots\ldots\ldots\ldots \quad (9.)$$

And these two are completely equivalent to the six equations commonly employed.

(104.) The line represented by the symbol $\mathrm{A}\omega_1\alpha + \mathrm{B}\omega_2\beta + \mathrm{C}\omega_3\gamma$ has a remarkable relation to the instantaneous axis $\omega_1\alpha + \omega_2\beta + \omega_3\gamma$, which may be thus interpreted. Suppose the rigid body to undergo a *distortion* or *unequal expansion* of such a nature, that all lines *in it* parallel to α become A times longer than before, all lines parallel to β, B times longer, and all lines parallel to γ, C times longer. The effect of this will be to convert the unit α into $\mathrm{A}\alpha$, β into $\mathrm{B}\beta$, γ into $\mathrm{C}\gamma$; and thus the line $\omega_1\alpha + \omega_2\beta + \omega_3\gamma$ will be converted into $\mathrm{A}\omega_1\alpha + \mathrm{B}\omega_2\beta + \mathrm{C}\omega_3\gamma$. This latter line, therefore, I may call the *Distorted Instantaneous Axis.*

(105.) The *distortion* here alluded to is one of great importance to be noted, because it indicates an operation which has immediate connection with many remarkable physical phenomena as well as with various theories in Solid Geometry. As regards its geometrical meaning, if we conceive the rigid body to be a solid composed of spherical shells having a common centre at the origin, each shell will be converted into an ellipsoid by the distortion. The sphere whose radius is unity will be changed into an ellipsoid whose axes are $\mathrm{A}\alpha, \mathrm{B}\beta, \mathrm{C}\gamma$; and the axes of the other ellipsoids will be parallel and proportional to these.

The line represented by the symbol

$$\mathrm{A}\alpha + \mathrm{B}\beta + \mathrm{C}\gamma$$

is an important determining element. If we assume ω' to denote what ω becomes in consequence of the distortion, it may be easily seen that ω' is a *distributive function* of ω and $\mathrm{A}\alpha + \mathrm{B}\beta + \mathrm{C}\gamma$; and from this fact a number of curious symbolical relations may be deduced. But I must not dwell upon this subject of distortion now further than my immediate purpose requires.

(106.) Using ω' for brevity to denote the *distorted instantaneous axis*,

$$\mathrm{A}\omega_1\alpha + \mathrm{B}\omega_2\beta + \mathrm{C}\omega_3\gamma,$$

I may observe that the equation (8.), that is,

$$\frac{d\omega'}{dt} = \Sigma m \mathrm{D}u . \mathrm{U}, \quad \ldots\ldots\ldots\ldots \quad (10.)$$

* Observing that $\frac{d\alpha}{dt} = \mathrm{D}\omega . \alpha$, $\frac{d\beta}{dt} = \mathrm{D}\omega . \beta$, $\frac{d\gamma}{dt} = \mathrm{D}\omega . \gamma$, we find the coefficient of γ in the first member to be

$$\mathrm{C}\frac{d\omega_3}{dt} + (\mathrm{B} - \mathrm{A})\omega_1\omega_2;$$

and putting $\mathrm{U} = \mathrm{X}\alpha + \mathrm{Y}\beta + \mathrm{Z}\gamma$, we find, in the second member,

$$\Sigma m(x\mathrm{Y} - y\mathrm{X}).$$

gives the velocity of the point (ω') *in space*, the differential letter, d, denoting *absolute* change of position. But it is often important to determine the motion of this point *relatively to the rigid body*; and this may be done as follows:—

Let us assume δ to denote *relative* change of position with reference to the rigid body; then it is evident that

$$\frac{d\omega'}{dt}=\frac{\delta\omega'}{dt}+\mathrm{D}\omega.\omega'; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

for the velocity of the point (ω') is the resultant of two velocities, namely, that relative to the body, and that arising from the motion of the body; the former is $\frac{\delta\omega'}{dt}$, and the latter, by (9.), is $\mathrm{D}\omega.\omega'$.

Hence, and by (10.), we find

$$\frac{\delta\omega'}{dt}=\Sigma m\mathrm{D}u.\mathrm{U}-\mathrm{D}\omega.\omega'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

This equation gives the velocity of the point (ω') *relatively to the rigid body*.

Now it is clear that if we can solve (10.) and (12.) the motion of the rigid body is determined; for we shall then know (by (12.)) how the line ω' moves *in the rigid body*, and by (10) how ω' moves *in space*; and thus, by the intervention of ω', we shall obtain the motion of the rigid body in space.

(107.) As an example of this I shall take the case of the earth attracted by the sun, and point out briefly how (10.) and (12.) determine the motion of the polar axis. In this case $\mathrm{A}=\mathrm{B}$, and $\mathrm{C}=(1+\lambda)\mathrm{A}$, when λ is a small number; also the instantaneous axis ω very nearly coincides in direction with the polar axis γ. Hence, and by art. 89, we have

$$\omega'=\mathrm{A}(\omega+\lambda\omega_3\gamma)$$

$$\Sigma m\mathrm{D}u.\mathrm{U}=\mathrm{A}\frac{3m'}{r'^5}\mathrm{D}u'.(u'+\lambda z'\gamma).$$

Here u' denotes (symbolically) the sun's distance, r' is the magnitude of u', m' the sun's mass.

Thus, observing that $\mathrm{D}u'.u'$ and $\mathrm{D}\omega.\omega$ are zero, (10.) and (12.) become

$$\frac{d\omega}{dt}+\lambda\frac{d(\omega_3\gamma)}{dt}=\lambda\frac{3m'}{r'^5}z'\mathrm{D}u'.\gamma$$

$$\frac{\delta\omega}{dt}+\lambda\frac{\delta(\omega_3\gamma)}{dt}=\lambda\frac{3m'}{r'^5}z'\mathrm{D}u'.\gamma-\lambda\omega_3\mathrm{D}\omega'.\gamma.$$

In the terms multiplied by λ we may approximate on the supposition that γ is fixed and $\omega=n\gamma$, where n is the earth's angular velocity about its polar axis. This reduces the two equations to

$$\frac{d\omega}{dt}=\lambda\frac{3m'}{r'^5}z'\mathrm{D}u'.\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

$$\frac{\delta\omega}{dt}=\lambda\frac{3m'}{r'^5}z'\mathrm{D}u'.\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

Now let us take new directions $(\alpha', \beta', \gamma')$ so, that γ' shall coincide for a moment with the polar axis γ, while the plane $(\alpha'\gamma)$ contains the sun's distance u'; then

$$u' = x'\alpha' + z'\gamma$$
$$\mathrm{D}u' \cdot \gamma = -x'\beta';$$

wherefore (14.) becomes

$$\frac{\delta\omega}{dt} = \left(-\lambda \frac{3m'}{r'^5} x' z'\right)\beta'.$$

The coefficient of β' here, being multiplied by λ, may be regarded as invariable during one day, inasmuch as x' and z' take a year to go through their values: also, since δ implies that the earth is considered as fixed, the sun, and therefore the direction β', must be supposed to revolve about γ from east to west, through 360° in the day. The velocity $\frac{\delta\omega}{dt}$ therefore is constant in magnitude but changes its direction (which is always perpendicular to γ) uniformly through 360° in the day. It appears therefore that the point (ω) describes a daily circle, and therefore the line ω describes a daily cone about γ. From this it follows (observing that ω and γ make a very small angle with each other), that the mean daily angular motion in space of γ and that of the direction of ω (manifestly $\frac{\omega}{n}$ very nearly) are identical. Wherefore

$$\frac{d\gamma}{dt} = \frac{1}{n}\frac{d\omega}{dt} = \frac{\lambda}{n}\frac{3m'}{r'^5} x' \mathrm{D}u' \cdot \gamma, \text{ by (13.)};$$

or, since $x' = u' \times \gamma$,

$$\frac{d\gamma}{dt} = \frac{\lambda}{n}\frac{3m'}{r'^5}(u' \times \gamma)\mathrm{D}u' \cdot \gamma. \quad \ldots\ldots\ldots\ldots \quad (15.)$$

If now we assume γ' to be at right angles to the plane of the ecliptic, and α' to point towards the first point of Aries, we have

$$u' = r'(\alpha' \cos n't + \beta' \sin n't),$$

where $n't$ is the sun's longitude, n'^2 being $\frac{m'}{r'^3}$.

Wherefore, observing that γ is at right angles to α', we find

$$\frac{d\gamma}{dt} = \frac{3n'^2\lambda}{n}(\gamma \times \beta') \sin n't \,(\cos n't \mathrm{D}\alpha' \cdot \gamma + \sin n't \mathrm{D}\beta' \cdot \gamma).$$

If we integrate this between the limits 0 and $\frac{2\pi}{n'}$, we find the *annual variation of* γ, which therefore is

$$\frac{3n'\lambda\pi}{n}(\gamma \times \beta')\mathrm{D}\beta' \cdot \gamma.$$

This represents in magnitude and direction the actual space described by the point (γ) in one year, *i. e.* the angular motion of the pole, or the precession. If ϖ denote the obliquity of the ecliptic,

$$\gamma = \gamma' \cos \varpi + \beta' \sin \varpi,$$

and $$\therefore \gamma \times \beta' = \sin \varpi$$
$$D\beta' . \gamma = \alpha' \cos \varpi .$$

Wherefore the annual precession of the pole is

$$\left(\frac{3n'\lambda\pi}{n} \cos \varpi \sin \varpi\right)\alpha',$$

α' indicating that its direction is perpendicular to the solstitial colure, and retrograde as regards the sun's motion.

I have gone through this example because in every step it shows the use of the symbolic forms.

VI. Application of the Symbolic Forms to Physical Optics.

(108.) The use of the symbolic forms $u.v$ and $u \times v$ in Physical Optics is very remarkable; but as this paper is already so long, I can only just allude to the subject.

In the Transactions of the Cambridge Philosophical Society I have shown (in a paper read March 17, 1847), that if v denote the displacement at the point (u) of an uncrystallized medium, where

$$u = x\alpha + y\beta + z\gamma$$
$$v = \xi\alpha + \eta\beta + \zeta\gamma,$$

and if A and B be two constants (namely, the coefficients of direct and transverse elasticity); then

$$\frac{d^2 v}{dt^2} = B\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}\right)v + (A - B)\left(\alpha\frac{d}{dx} + \beta\frac{d}{dy} + \gamma\frac{d}{dz}\right)\left(\frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz}\right).$$

And this result I obtained by merely considering the *disarrangement* of the medium about the point u, without any assumption respecting the constitution of the medium, except that it possessed direct and transverse elasticity.

Now if we employ the letter Ω to denote the operation

$$\alpha\frac{d}{dx} + \beta\frac{d}{dy} + \gamma\frac{d}{dz},$$

the above equation, by the aid of the symbolic forms, immediately assumes the simple form

$$\frac{d^2 v}{dt^2} = A\Omega(\Omega \times v) - B(D\Omega .)^2 v. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

The symbol Ω has a very important signification when written before any quantity, U, which is a function of x, y and z: for the *direction* of the line ΩU is that direction *perpendicular to which there is no variation of* U; while the magnitude of ΩU is the *rate of variation* of U *in that direction, i. e.* as we pass from point to point of the medium in the direction of ΩU.

Again, $\Omega \times v$ denotes the *rarefaction* of the medium, at the point (u), resulting from

the displacements represented by v; while $\Omega.v$ denotes, *in magnitude and plane*, the *lateral disarrangement* of the medium.

It is clear, therefore, that the symbolic forms $u.v$ and $u\times v$ must be of great use in Physical Optics; indeed the facility they give of following out investigations respecting undulatory movements is so great, that the whole subject of reflexion and refraction, in crystallized as well as in uncrystallized media, and the mathematical explanations of the phenomena connected with polarization, double refraction, &c., may be reduced to a state of simplicity which could hardly be expected in such a difficult subject.

(109.) In the paper above alluded to, I obtained also the equation of vibratory motion generally, for any crystallized medium, without any of those assumptions which mathematicians have found it necessary to make in order to render the investigation manageable; especially, without assuming the vibrations of a plane polarized ray to be *in* the plane of polarization, which appears to me to be a highly objectionable assumption. By the aid of the symbolic forms, the general equation of vibratory motion, where the transmission of transverse vibrations is possible, is thus expressed:

$$\frac{d^2v}{dt^2}=\left(A_1\alpha\frac{d}{dx}+A_2\beta\frac{d}{dy}+A_3\gamma\frac{d}{dz}\right)(\Omega\times v)$$

$$+D\Omega.\left\{\left(B_3\frac{d\eta}{dz}-B'_3\frac{d\zeta}{dy}\right)\alpha+\left(B_3\frac{d\zeta}{dx}-B'_1\frac{d\xi}{dz}\right)\beta+\left(B_1\frac{d\xi}{dy}-B'_3\frac{d\eta}{dx}\right)\gamma\right\}. \quad . \quad . \quad (2.)$$

Here A_1, A_2, A_3 are coefficients of *direct elasticity*, corresponding to A in equation (1.); and B_1, B_2, B_3, &c. are six coefficients of *transverse elasticity*, corresponding to B in (1).

Fresnel's hypothesis, that the vibrations of a plane polarized ray are *perpendicular* to the plane, makes

$$B_1=B'_1, \qquad B_2=B'_2, \qquad B_3=B'_3,$$

while the hypothesis, that the vibrations are *in* the plane of polarization, makes

$$B_1=B_2, \qquad B_2=B'_3, \qquad B_3=B_1.$$

On the former hypothesis the equation becomes

$$\frac{d^2v}{dt^2}=\left(A_1\alpha\frac{d}{dx}+A_2\beta\frac{d}{dy}+A_3\gamma\frac{d}{dz}\right)(\Omega\times v)-(D\Omega.)^2(B_1\xi\alpha+B_2\eta\beta+B_3\zeta\gamma); \quad . \quad . \quad . \quad (3.)$$

while, on the latter, it becomes

$$\frac{d^2v}{dt^2}=\left(A_1\alpha\frac{d}{dx}+A_2\beta\frac{d}{dy}+A_3\gamma\frac{d}{dz}\right)(\Omega\times v)$$

$$-(D\Omega.)\left\{B_1\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\alpha+B_2\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)\beta+B_3\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)\gamma\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

For transverse vibrations the rarefaction $(\Omega\times v)$ (see above) is zero, which further simplifies (3.) and (4.). By equating the coefficients of α, β, γ in (4.), thus simplified, we obtain MacCullagh's three equations. The equation (3.) coincides in every way with Fresnel's theory.

The conception of *distortion* alluded to in Sect. V. applies to (3.) in a very remarkable manner; for if we put

$$A_1\alpha\frac{d}{dx}+B_1\beta\frac{d}{dy}+C_1\gamma\frac{d}{dz}=\Omega',$$

$$B_1\xi\alpha+B_2\eta\beta+B_3\zeta\gamma=v',$$

then Ω' is Ω *distorted* by the line $A_1\alpha+A_2\beta+A_3\gamma$; and v' is v *distorted by* the line $B_1\alpha+B_2\beta+B_3\gamma$. In using the expression "distorted by," I anticipate a signification which I hope to explain in a future paper; but the fact is, Ω' is a *distributive function* of Ω and $A_1\alpha+A_2\beta+A_3\gamma$ (see art. 105), and therefore, symbolically, Ω' is Ω *multiplied by* $A_1\alpha+A_2\beta+A_3\gamma$.

Now the equation (3.) becomes

$$\frac{d^2v}{dt^2}=\Omega'(\Omega.v)-(D\Omega.)^2v',$$

or, for transverse vibrations, simply

$$\frac{d^2v}{dt^2}=-(D\Omega.)^2v'.$$

But I must reserve the consideration of this remarkable equation, merely remarking here that it shows very clearly how the force brought into play by the disarrangement of a medium resulting from transverse vibrations is altered by the crystallization. In the uncrystallized medium the force is, by (1.),

$$-(D\Omega.)^2(B\xi\alpha+B\eta\beta+B\zeta\gamma),$$

and in the crystallized medium it is

$$-(D\Omega.)^2(B_1\xi\alpha+B_2\eta\beta+B_3\zeta\gamma).$$

In one case it is found by performing the operation $-(D\Omega.)^2$ on v, in the other by performing the same operation on v *distorted* as by the threefold expansion of the medium, consisting of a drawing out in the direction α in the proportion of B_1 to B, in the direction β, of B_2 to B, and in the direction γ, of B_3 to B.

Upper Norwood, Surrey,
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